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ON THE FORM OF THE MILKY WAY.

By JOHN RICHARD SUTTON, B.A.Cantab.

THE late Mr. Proctor shone as a critic. One gets the same impression from a literary review by him as from one of Matthew Arnold's literary reviews, viz. that in most cases common sense has little more to say. His essays on the star-work of the two Herschels are among the most masterly in the language, and are destined, no doubt, to be read for generations. But his original speculations are not always so happy. His genius was iconoclastic rather than creative; and although few who have appreciated the significance of the work of Michell and the school he set up will question the fact that Proctor has successfully summed up, once for all, the case against Sir William Herschel's "cloven-flat-disc theory" of the Milky Way, yet it is doubtful if the same success can be claimed for the "spiral theory," which he advanced as comprehending all the results of observation on the stars and nebulæ.

It would be out of place here to enter into a discussion of the stellar researches of the Herschels. It will be sufficient to say that the elder Herschel assumed the stars to be, *on the average*, scattered more or less uniformly throughout the visible universe, and hence, by counting the number of stars visible in any one direction, he was, as he thought, able to arrive at a pretty fair approximation to the depth of the star-system in that direction. [It is not hard to see that when the telescope is fixed on any point in the sky, the boundaries of vision trace out a cone in the celestial sphere of which the eye is the vertex.] The younger Herschel seems to have accepted his father's results with little more than a twinge of doubt, although it is not so certain that he placed a very great faith in the trustworthiness of the star-gauging methods from which they were derived; for he could also see, brilliant mathematician as he was, that Michell's application of the doctrines of probability had practically set at rest the fact that *in general* star-grouping was physical and real, and not merely optical, and hence that the stars were by no means uniformly distributed. Still, the doctrine of probabilities had not sufficient hold upon his mind to cause him to give up the delightfully systematic results of star-gauging, to say nothing of the paternal authority by which the old theory was backed.

Proctor, however, took higher ground. In his hands Michell's researches were, so to speak, born again. The star-groups were physical facts (which, indeed, Sir William Herschel had never doubted, though he left it out of account); the constellations were realities of association (of which Sir William Herschel had never even dreamed); immense streams of stars, physically connected *inter se*, were to be traced across the sky; and, last of all, the Milky Way itself was a great stream of stars*—and not a solid disc—to which all the other streams and groups were subordinate. These different conclusions were advanced from time to time for many years, and, taken together, they comprise an analysis of stellar phenomena whose immense value cannot be denied.

It would be an agreeable task for me to give here, as far as possible, an account of Proctor's very successful work among the stars. But such a course is just now out of the question; moreover, most readers whom this paper may concern will have read his original papers. I shall content myself by remarking that he very early concluded that all available evidence pointed in one direction, namely, that the characteristic feature of our universe, the Milky Way, was one long stream of stars of a spiral form, whose plane passed very nearly through the sun's place. He says: "It is very clear what views we are to form respecting the Milky Way. If the galaxy is, *first*, a clustering aggregation separated from us by an interval comparatively clear of small stars; *secondly*, so shaped that the cross-section of the stream is everywhere not far from a roughly circular figure; and, *thirdly*, associated

* Sir William Herschel's language, even in his early papers of 1784 and 1785, and much more in his later papers on the Construction of Heavens, published in the *Phil. Trans.* of 1802 and 1811, clearly prove that he fully appreciated the fact that there are actual clusters and streams of stars, as well as vacant spaces, in the heavens. It is evident, also, that he recognized the connection between the distribution of stars and the distribution of nebulæ. As early as 1802 Sir W. Herschel's language clearly shows that he considered the Milky Way to be a roughly circular ring-shaped region, in which the stars are more thickly grouped than in the space immediately around the sun. He says, in a paper published in the *Phil. Trans.* in 1802: "The stars we consider as insulated are also surrounded by a magnificent collection of innumerable stars, called the Milky Way, which must occasion a very powerful balance of opposite attractions to hold the intermediate stars in a state of rest. For though our sun, and all the stars we see, may truly be said to be in the plane of the Milky Way, yet I am convinced, by a long inspection and continued examination of it, that the Milky Way itself consists of stars very differently scattered from those which are immediately about us." Sir John Herschel also clearly recognized the close clustering of stars in the region of the Milky Way. Mr. Proctor was well aware that both the Herschels had recognized the existence of star-streams as well as groups of nebulæ. But I agree with Mr. Sutton that neither Sir William nor Sir John Herschel proceeded to the conclusion which should have logically followed, namely, that their method of star-gauging could not be relied upon for determining the form of the stellar universe.—A. C. RANYARD.

very closely with the bright stars seen in the same field of view, then must its structure be somewhat as shown in the figure. . . . It will be seen at once how, to an observer placed at S, the various features of the Milky Way can be accounted for by this figure. Towards 1 would lie the gap in Argo; towards 3, 4, 5, two branches, one faint, and in part evanescent through vastness of distance, the other forming the brightest part of the spiral; towards 6 the projection in Cepheus; towards 7 the faint part of the Milky Way in Gemini and Monoceros. The Coal-sacks would be simply accounted for by conceiving that branches seen towards the same general direction, but at different distances, do not lie in the same general plane, and so may appear to interlace upon the heavens. We are not only justified in supposing this, but forced to do so by the way in which the stream of milky light is observed to *meander* on its course athwart the heavens. The branching extensions serve very well to account for the appearance of the Milky Way between Cepheus and Ophiuchus, where the interlacing branches and the strange convolutions and clustering aggregations described by Sir John Herschel are chiefly gathered."—*Other Worlds*, p. 250.

Proctor's affection for this spiral is remarkable. It was advanced first of all with some caution as a conformation which seemed to account for *all* the known peculiarities of structure in the galaxy—in short, it supplied a useful working theory. For example, he makes the following admission: "I would not have it understood, however, that I at all insist on the general shape of the spiral shown in the figure. On the contrary, that curve is only one out of several which might fairly account for the observed appearance of the Milky Way; and I have often felt inclined to doubt whether a single spiral of this sort is in reality the best way of accounting for the observed appearance of the galactic zone. What I do insist upon as obviously forced upon us by the evidence is that (1) the apparent streams formed by the Milky Way upon the heavens indicate the existence of real streams in space; and (2) that the lucid stars seen on the stream are really associated with the telescopic stars which form, so to speak, the body of the stream. Whether that stream form a single spiral or several, or whether, instead of spirals, there may not be a number of streams of small stars, placed at different distances from us, and lying in all directions round the medial plane of the galaxy, but more or less tilted to that plane (the sun not lying within any one of the streams), are questions which can only be resolved by the systematic scrutiny of this wonderful zone."

But just as Sir William Herschel's working-theories of star-distribution came eventually to be regarded as established facts, so in course of time, without any more apparent grounds, Proctor came to regard his theory with increasing confidence. Scattered throughout his writings will be found very trustful remarks on the conformation of "that strange spiral."

Without doubt, if the star-groups, such as the Pleiades, are real, and not merely caused by stars far apart, though near the same visual-line, being seen projected into a small area of the celestial sphere (and Michell's, not to mention later results, seem to make it certain enough); if the star-streams are real, and not mere optical coincidences; if the manifold signs of association and dissociation among the stars are what common-sense would teach; then it is as certain as the doctrine of chances can make it that the Milky Way is a stream, or collection of streams, of stars. So much, but no more, Proctor has proved. What he has not proved, nor even made possible, is that the Milky Way partakes in any way of a spiral form.

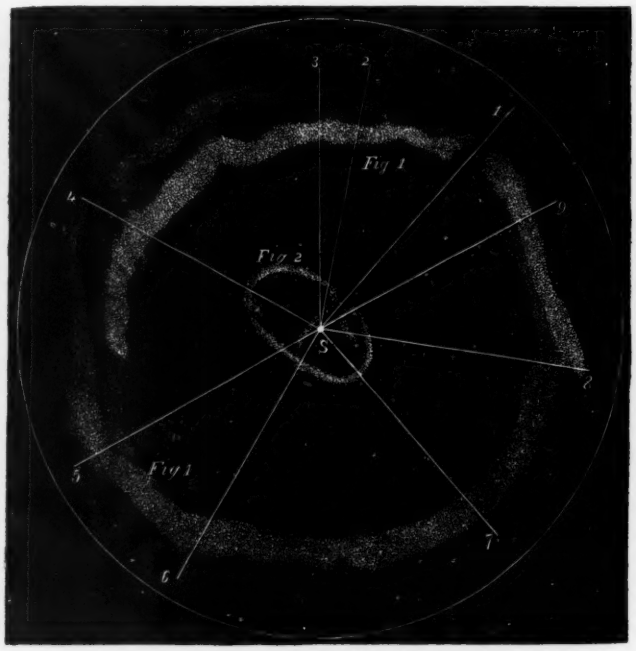

Fig. 1.—The Milky Way as seen in the Heavens.
Fig. 2.—The Spiral Stream which Mr. Proctor assumed represented the actual form of the Milky Way.

Let us see what the brightness of the Milky Way can tell us. Proctor assumed the brightness of its various parts would be a good rough test of their relative distances—a curious mistake for one so conversant with the laws of brightness. [It may be noted that Sir John Herschel erred in the same direction.] Now it may be proved that brightness alone is no test of distance. For let a be the apparent area of any small portion of the Milky Way at some distance taken as unity, and containing n stars, λ the average amount of light received from each star, β the average brightness of the area. If this area be removed to distance d, the light received from each star will be reduced to $\frac{\lambda}{d^2}$; but the apparent area containing the n stars will be reduced to $\frac{a}{d^2}$, and hence the stars are apparently compressed into a smaller area in the same proportion as their light is reduced. β, therefore, remains unaltered. The apparent breadth of the galaxy would be a much safer test of its distance than the brightness, although its actual breadth may vary between very wide limits. As a matter of fact, the broadest parts of the Milky Way are, on the whole, the faintest; whereas in its narrowest part, at the "isthmus" leading into the Southern Cross, it is almost at its maximum brilliancy.

During the course of his long review of the elder Herschel's cloven-flat-disc theory, Proctor pointed out again and again the remarkable tendency of the brighter stars to congregate along the course of the Milky Way and its branches. This feature is so noticeable in the

southern hemisphere that on a bright moonlight night, when the fainter lucid stars are invisible, the position of the Milky Way can be traced by the condensation of the brighter stars alone along its course. Now, there is no antecedent reason why the naked-eye stars should collect upon one part of the Milky Way and not upon another; and hence there is no reason why the fainter parts of the galaxy—assuming them for the moment to be the most distant—should not have collected fourth and fifth magnitude stars in the same way as the brighter and (therefore assumed) nearer parts have collected the brighter magnitudes. But we should not expect to find many of the brightest stars on the fainter and more distant parts. Yet it is a fact that there are stars on the (assumed) more distant streams, and obviously associated with them, as bright as those on the (assumed) nearer ones. Moreover, between the two streams, where the Milky Way is double, and in the Coal-sacks, scarcely a single lucid star is to be seen. This is a singular circumstance. If the spiral theory, or any modification of it as suggested by Proctor, were true, we should expect more, or at least as many, bright stars between the two branches where the Milky Way is double, and in the Coal-sacks, as upon the borders of the Milky Way in any other place. On the other hand, if we regard the two branches as lying at very nearly the same distance from the Sun, we can see at once that the cause which separated them, or keeps them apart—whatever the agency may be scarcely matters in this inquiry—would also *drain*, as it were, the lucid stars from the space between. Add to this the circumstance that the meanderings of one branch correspond to those of the other, and that in one place one branch is made up of alternating bright and faint patches corresponding to faint and bright patches respectively on the other, then we have strong presumptive evidence that the two branches are closer together than any spiral conformation would admit.

All this leads us to conclude that the Milky Way is much as it seems to be at first, namely, a very complex stream of stars roughly hoop-like, and not spiral-like, in form. A piece of old rope thrown into a circular shape represents the Milky Way very well. The frayed ends not quite meeting represent the fan-shaped expansions of the stream on each side of the rift in Argo (a spiral theory does not explain why these expansions should occur just here). A strand untwisted of nearly half the length of the rope, and divided in the middle, would represent the divided branch running from Cygnus through Ophiuchus and Scorpio. Smaller departures would represent the lateral extensions in Cepheus and Perseus. These may be of any size; and it scarcely needs Proctor's assumption that a vast void separates the galactic stars from the faint streams discovered by Sir John Herschel to account for them. This faintness is very likely caused either by sparcity of distribution or by the smallness of the stars (as we have seen brightness is no test of distance so long as light is not lost in its passage through space); indeed, it is pretty clear that the Milky Way is made up of stars of all orders of size and brightness. Local unravellings in the rope would correspond to the Coal-sacks in Crux and Cygnus, and to the lacunæ in Scorpio and elsewhere.

In conclusion, let me add that the aspect of the Milky Way in Argo gives a very strong impression that the stream has been forcibly torn asunder at this place, an impression which is to some extent corroborated by the positions of the bright stars round about, and by the manifold signs of decomposition exhibited by the galaxy in Scorpio and the adjacent constellations.

GIANT BIRDS.

By R. Lydekker, B.A.Cantab.

THE only birds existing at the present day which in any sense merit the epithet "gigantic" are the Ostriches of Africa and Arabia, the Rheas of South America, the Cassowaries of Papua and North Australia, and the Australian Emus; and even the largest of these—the male Ostrich—seldom exceeds seven feet in height. The researches of palæontologists have, however, revealed to us that these four groups are but the solitary survivors of a considerably more extensive assemblage of Giant Birds which was once spread over a large portion of the globe, and some of whose members as much surpassed the Ostrich in size as the latter exceeds the Rheas in this respect. Indeed, with our present knowledge of the meaning of the geographical distribution of animals, the very circumstance that the existing Giant Birds are all more or less closely allied, and are found scattered over the globe in areas widely separated and totally disconnected from one another, would of itself have been amply sufficient to indicate that they are the remnants of a group which was at one time of much larger extent, and inhabited regions where such creatures are now unknown. It is unfortunately the case that there are still many gaps in the chain which should link all the existing Giant Birds together, but we may confidently hope that the progress of geological research will little by little reduce the number and length of these gaps.

All the Giant Birds, it may be observed, both living and extinct, are linked together by their incapacity of flight, and the consequent absence of that strong bony ridge or keel which we may observe on the breast-bone of a duck or a fowl, and the presence of which is essential to form a firm support for the powerful muscles required to move the wings. Whether, however, this incapacity for flight is a feature which was always possessed by the Giant Birds, or whether it has been gradually acquired by disuse, is a question which has seriously exercised the minds of those best fitted to decide it, and, since it is still unanswered, may be put aside on this occasion. Moreover, in saying that the Giant Birds are all incapable of flight, it must by no means be inferred that all birds in that condition have any affinity to this group. On the contrary, precisely the opposite is the case, since in the extinct Dodo of the Mauritius we have an instance of a huge pigeon which had evidently lost the power of flight; and the superficial deposits of New Zealand have yielded the remains of a large rail and a goose which were in the same predicament. These and other flightless birds differ, however, essentially in several parts of their organization from the true Giant Birds, and thus have no sort of connection with the subject of this article. There are, however, certain birds, namely the little Kiwis of New Zealand, which, although by no means entitled to rank as Giant Birds in the proper sense of that term, yet as being closely related to the typical members of that group, must find a place therein. Before, however, we can consider the fossil members of this group it is necessary that we should have some idea of the general structure of the leg of a bird, since it is this part of the skeleton which is most commonly met with in a fossil condition, and which affords the most important clue as to the size and affinities of the bird to which it belonged. Some observations on this point have already been made in the article on Giant Reptiles; but since those observations were mainly directed to showing the resemblance between

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the leg of a bird and that of a reptile, they are not well suited to our present purpose.

A bird's leg, then, as shown in Fig. 1, is composed of four segments; the upper short one corresponding to the human thigh-bone, and the lowest representing the toes, which are composed of several small bones. Between these two segments are the two long and slender bones shown in the figure. The upper and longer of these two corresponds with the human leg-bone, *plus* the knuckle-bone welded on to its lower end. The lower and shorter bone, of which another example is shown in Fig. 2, is a very remarkable bone indeed, and may be conveniently called the cannon-bone. It is really composed of three separate long bones, of which the ends remain free at the lower extremity and carry the toes, and also of the lower part of the ankle welded on to the upper end. The middle long bone corresponds exactly with the cannon-bone of a horse, the nature of which has been explained in the article on "Rudimental Organs"; and the whole compound bone would correspond with the metatarsus of the extinct three-toed horse known as the Hipparion, if its three metatarsals were welded together, and these again with the bones of the lower half of the ankle. It will, accordingly, be evident that a bird is an odd-toed animal like a horse (that is to say, the toe representing the middle one of the typical five is symmetrical in itself and larger than either of the others), having a cannon-bone, but no separate ankle-bones; the upper ankle-bone having become welded on to the leg-bone, and the lower ones similarly united to the cannon-bone. In these respects, therefore, a bird is a very specialised kind of creature, as departing widely from the original type. With these necessary anatomical explanations, we shall be in a position to enter on the subject of the extinct Giant Birds.

Fig. 1.—The Bones of the Right Leg of the Giant Moa. About $\frac{1}{13}$ nat. size. (*After Owen.*)

Fig. 2.—Front and Back Views of the Left Cannon-bone of a Partridge.

The first of these extinct birds brought to the notice of the scientific world were the Moas of New Zealand, in which islands the largest existing representatives of the group are the diminutive Kiwis. The original determination of the former existence of these giant birds affords, indeed, an interesting instance of the certainty of anatomical deductions, when made with proper care and sufficient knowledge. Thus, many years ago a man brought to Sir R. Owen the broken shaft of the thigh-bone of some large animal, which he stated had been obtained from New Zealand, where the natives believed that similar bones were those of a large eagle. The specimen was a somewhat unpromising one, but after careful comparison the Professor confidently pronounced that it belonged to an extinct bird considerably larger than any ostrich, for which he proposed the name of *Dinornis*. Other specimens soon after brought to this country triumphantly established the correctness of this bold identification, and showed that giant birds far surpassing in size any previously known must have existed at a comparatively recent date, and in extraordinary numbers in New Zealand. In the swamps—especially the well-known one of Glenmark, near Canterbury—these bones, and in some cases nearly entire skeletons, are very abundant; while in caves there have been obtained not only parts of skeletons with the skin still adhering to them, but even well-preserved feathers, and broken egg-shells. Although the Maoris well know that these remains belonged to gigantic birds, and give them a name of which the word Moa is generally considered to be a corruption, yet there is some difference of opinion as to whether their ancestors ever saw these birds in the flesh; some authorities considering that they were killed off by the race which is believed to have inhabited New Zealand before the advent of the Maoris. Still, in any case, Moas must have existed up to a very late epoch; and it is even said that the "runs" made by them were visible on the sides of the hills up to a few years ago, and may, indeed, still be so.

The leg-bone of a Moa may be at once known from that of all living Giant Birds by the circumstance that on the front surface of its lower end, immediately above the knuckle-bone, there is a narrow bar of bone forming a bridge over a small groove (this being indistinctly shown in Fig. 1). In the Giant Moa, of which the leg is represented in Fig. 1, the leg-bone attains the enormous length of one yard, and in an allied species from the South Island its length is upwards of 39 inches. The cannon-bone (as may be seen in the figure) is comparatively long and slender, and is more than half the length of the leg-bone. A skeleton of a smaller individual in the Natural History Museum has an approximate height of $10\frac{1}{2}$ feet, and we may thus conclude that the larger birds did not stand less than 12 feet. There were other species of true Moas, of about the dimensions of a large male ostrich, although of stouter build; and resembling the larger birds in having only three toes to the feet, and not the slightest trace of a wing.

Alongside of these giants there were, however, other species of much smaller size, in which there were four toes to each foot, and the cannon-bone was relatively much shorter. Thus the Dwarf Moa, of which the Natural History Museum possesses a complete skeleton, was not more than three feet high, while Owen's Moa was of still smaller dimensions. There were also other species of this group nearly as large as an ostrich.

Perhaps, however, the most remarkable of all these birds is the Elephant-footed Moa, which, although by no means equal in height to the Giant Moa, was of much more massive build. In this extraordinary bird the leg-bone is much shorter and thicker than in the Giant Moa, while the cannon-bone is so short and thick that it almost loses the character of a "long bone." In one unusually large example of the last-named bone, while the length is only $9\frac{1}{2}$ inches, the width at the lower end is upwards of $6\frac{1}{2}$. By the side of such a bone the cannon-bone of an ox looks small and slender, and the effects of a kick from such a leg can be better imagined than described.

The total number of kinds of Moa inhabiting New Zealand was probably at least fifteen, and, from the enormous accumulations of their bones found in some districts, we may assume that these creatures were extremely common, and probably went about in droves. Nothing like this bird-fauna is known in any other part of the world; and its exuberance may be probably explained by the absence of mammals from New Zealand, so that when the ancestors of the Moas once reached these islands they found a free field for unlimited development.

The nearest allies of the Moas are the small Kiwis; but whereas the latter have long pointed bills for probing in soft mud after worms, the bills of the Moa were short and broad like those of the Ostrich. Moreover, although the Kiwis have no wings visible externally, they retain rudimentary wing-bones, which have totally disappeared in the Moas. The plumage of the Moas appears to have been of the hair-like nature of that of the Kiwis. Since the latter differ from the Ostriches in that the females are larger than the males, we may assume that the same condition obtained among the Moas. The Kiwis are further remarkable for the enormous proportionate size of their eggs; and if anything like the same relative proportions held good with those of the Moas, the egg of the Giant Moa must have been of stupendous dimensions. It is, however, probable that the eggs of the larger Moas were relatively smaller than those of the Kiwis.

Passing to Australia, we find in the superficial deposits remains of a bird as large as some of the medium-sized species of Moa, but at once distinguished by the absence of a bridge of bone at the lower end of the leg-bone. This bird, which has been named *Dromornis*, is, however, as yet but very imperfectly known, so that we are to a great extent in the dark as to its affinities, though it was probably a distant giant relation of the Cassowaries.

Before we again meet with fossil giant birds we have to cross the whole extent of the Indian Ocean to Madagascar. Here there occurs the enormous bird known as the *Æpyornis*, the existence of which was first revealed by its eggs, which are found sunk in the swamps, but of which bones—mostly imperfect—were subsequently discovered. One of these enormous eggs measures three feet in its longer circumference, and 2½ feet in girth; its cubic contents being estimated at rather more than two gallons. The leg-bone of this bird has no bony bridge at its lower end, and the cannon-bone (of which only a portion is known) is as wide as that of the Elephant-footed Moa, but is much longer and thinner. The natives search after the eggs of this bird by probing for them in the soft mud of the swamps with long iron rods.

The Moas, the Dromornis, and the Æpyornis indicate, then, three totally distinct groups of Giant Birds; and since their various habitats occupy islands on both sides of the Indian Ocean, it is a fair presumption that their common ancestors originally inhabited some part of the great continental mass of the Old World. Support is afforded to this hypothesis by the occurrence of the Ostriches on the west, and the Cassowaries and Emus on the eastern side of the same great ocean. Moreover, there is historic evidence to the effect that Ostriches, which are now confined to Africa and Arabia, formerly existed in Baluchistan and Central Asia; and since their fossil remains occur in the Pliocene deposits of Northern India, there is little doubt that at least this group of Giant Birds originated in the northern part of the Old World. Again, the Indian deposits already mentioned have also yielded remains of a bird differing from the Ostrich in having three in place of two toes, and thereby agreeing with the Cassowaries and Emus, to which it was doubtless allied, and thus indicating that these birds likewise had their original home on the great Euro-Asiatic continent, from whence they have gradually migrated southwards till they reached regions free from the large carnivorous mammals of the continents.

Looking back through the Tertiary rocks of Europe to see if we can find there traces of ancestral Giant Birds, it is not till we come to the Lowest Eocene, or period immediately below the London clay, that our search is rewarded. Here, however, both in England, France, and Belgium, we meet with limb-bones and other remains of Giant Birds, which, from their huge size, must almost certainly have belonged to the group under consideration. In this bird, which is known as *Gastornis*, the lower end of the leg-bone has a bony bridge, as in the Moas; and since this is a feature common to the great majority of flying birds, it suggests a community of origin between them and the Giant Birds; the loss of this bridge in the living members of the latter thus being an acquired character. Although we are still very much in the dark as to the real affinities of the *Gastornis*, yet it appears to be more nearly related to the Moas and the Dromornis than to any other birds, and it might, therefore, have well been one of the ancestors of the group.

This is at present the extent of our knowledge of the former distribution of Giant Birds; but it may be confidently expected that whenever the Tertiary formations of Northern Africa and Southern and Central Asia are fully explored, we shall be rewarded by the discovery of other kinds, which will tend to more or less completely connect together those at present known to us, and which will also show how these have gradually migrated, since the Eocene Period, from the great continental northern mass to those southerly areas wherein some have existed up to a comparatively late period, and where others still remain as the sole living witnesses in the Old World of a group which has all but passed away.

THE MAGIC SQUARE OF FOUR.

By T. Squire Barrett, F.S.S.

To treat fully of the square of 4 alone would take a good-sized volume. The ancient Egyptians and Pythagoreans, in their ignorance of mathematics, thought it so wonderful that a series of numbers could be arranged to add up alike, upward, across, or diagonally, that they regarded such combinations with superstitious veneration. We, however, know that it would be much more wonderful if magic squares could *not* be made. For example, considering the difficulty with which a person, without some knowledge of the subject, could make a magic square with an arithmetical series of sixteen numbers, it would naturally be thought that it could be done in very few ways. But it was shown by Frenicle that it could be accomplished in at least 880 ways. Nor is this surprising when we consider that there are nearly three billion (2,615,348,736,000) ways of arranging 16 things in the form of a square.

It is more than possible that Frenicle's list does not exhaust the number of such squares. I have never seen his collection, and was therefore rather surprised to find that I could make exactly the same number, but no more. Nevertheless, I should not like to say that others could not be constructed.

These 880 squares, consisting as they do of numbers in arithmetical progression, may obviously be classified according to the relative position of each pair of complementary numbers. When the numbers are the natural series from 1 to 16, each complementary pair will sum 17; 1+16, 2+15, 3+14, and so on.

This classification shows the existence of 12 different types, some of which are very curious. The most perfect of them is the *nasik* square. [For definition of a nasik magic square, see *foot-note*, in Knowledge, p. 277.] Of this type (which I call A) it can be mathematically proved that only 48 variations are possible. I give an example on the next page.

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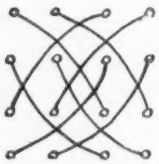
A

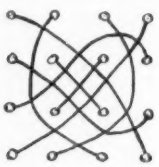


10	15	4	5
8	1	14	11
13	12	7	2
3	6	9	16

The diagram on the left shows the relative position of complementary numbers, a line in each case running from one to the other.

The remaining 11 types, with examples, are as follows :—

B

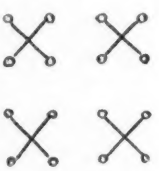


1	15	12	6
8	10	13	3
14	4	7	9
11	5	2	16

C

5	2	15	12
11	16	1	6
10	13	4	7
8	3	14	9

D



13	11	8	2
6	4	15	9
3	5	10	16
12	14	1	7

E

1	7	16	10
14	12	3	5
11	13	6	4
8	2	9	15

F

1	16	7	10
11	6	13	4
14	3	12	5
8	9	2	15

G

8	5	12	9
13	4	1	16
3	14	15	2
10	11	6	7

H

16	15	1	2
4	3	13	14
5	10	8	11
9	6	12	7

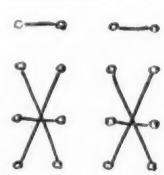
I

5	13	4	12
16	8	1	9
3	11	14	6
10	2	15	7

J

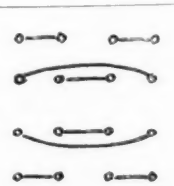
8	16	9	1
13	5	12	4
2	10	7	15
11	3	6	14

K



4	13	3	14
5	8	10	11
16	1	15	2
9	12	6	7

L



1	16	3	14
12	7	10	5
13	2	15	4
8	9	6	11

The above types are distributed amongst the 880 squares in the following proportions :—

Of type	A	there are	48	varieties
,,	B	,,	48	,,
,,	C	,,	272	,,
,,	D	,,	96	,,
,,	E	,,	96	,,
,,	F	,,	96	,,
,,	G	,,	52	,,
,,	H	,,	8	,,
,,	I	,,	52	,,
,,	J	,,	52	,,
,,	K	,,	8	,,
,,	L	,,	52	,,
			880	

There is a close connection between some of the types, especially between those of which there is the same number of varieties. A simple transposition of the figures will in many cases convert one into another. As an instance of this, types G, I, J, and L are thus reciprocally convertible. If we take a G square and transpose the two middle rows, and then the two middle columns, it is turned into a square of the I type ; and if this I square has its numbers diagonally adjacent to each other (like those connected in diagram D) transposed, we get a square of type J. If, further, this J square has its middle rows and columns transposed, a square of type L is formed ; and, once more altering the result like diagram D, we finally get the same square of G with which we started, but turned upside down.

One way of estimating the number of squares of a given type is to decompose the numbers into two skeleton squares, each containing four numbers four times repeated. By way of example, let us decompose a square of type C, being the most numerous, putting the numbers 1, 2, 3, 4 into one and 0, 4, 8, 12 into the other skeleton square ; thus :—

1	6	11	6		1	2	3	4		0	4	8	12
15	12	5	2		3	4	1	2		12	8	4	0
14	9	8	3		2	1	4	3		12	8	4	0
4	7	10	13		4	3	2	1		0	4	8	12
	(1)					(2)					(3)		

(1) is a square of type C ; and (2) and (3) are two skeleton squares which on being added together produce (1).

Now, on examining the structure of the squares (2) and (3), we see that their own complementary pairs are arranged in accordance with the type of (1), that is C. In (2) the sum of each pair is 5 ; in (3) the sum is 12. On further examination of the squares, we may observe that the numbers may be transposed in various ways so as to produce different results, still belonging to the same type. Thus, in square (2) each 2 may be written 3, and *vice versâ*, and each 4 may be written 1, and *vice versâ*. This will give in combination with square (3) three additional squares of the same type, or four in all.

Furthermore, the numbers in square (3) may be transposed in similar manner, 4 for 8 and 0 for 12, and *vice versâ*, giving another 4 varieties, which, combined with the 4 varieties of the other skeleton square, give us 4×4, or 16 squares of the type. Again, the arrangement of the two skeleton squares may be reversed ; we may write down the 1, 2, 3, 4 in the way the 0, 4, 8, 12 are written down above, and *vice versâ*. This doubles the number of squares producible, giving us 16×2, or 32. Once more, a partial transposition may be made in the numbers of square (2). For example, the 2 and 3 may be transposed in the top and bottom rows, whilst those in the middle rows are undisturbed.

This partial transposition may be performed on whatever numbers occupy the middle cells of the top and bottom rows. This again doubles the number, bringing it up to 32×2, or 64.

Nor is this by any means all. There are two other ways of decomposing these squares. Instead of putting 1, 2, 3, 4 into one, and 0, 4, 8, 12 into the other skeleton square, we may put 1, 2, 5, 6 into one, and 0, 2, 8, 10 into the other ; or 1, 3, 5, 7 into one and 0, 1, 8, 9 into the other. Thus

1	2	5	6		0	2	8	10		1	4	13	16
5	6	1	2		10	8	2	0		15	14	3	2
2	1	6	5		10	8	2	0		12	9	8	5
6	5	2	1		0	2	8	10		6	7	10	11
	(4)					(5)					(6)		
1	3	5	7		0	1	8	9		1	4	13	16
5	7	1	3		9	8	1	0		14	15	2	3
3	1	7	5		9	8	1	0		12	9	8	5
7	5	3	1		0	1	8	9		7	6	11	10
	(7)					(8)					(9)		

The second set of series in (4) and (5), and the third set in (7) and (8), have just the same arrangement respectively as the numbers in (2) and (3). The resulting squares (6) and (9), it may be noticed, are different from (1) and different from each other. Using all these three sets of series therefore trebles the number of squares previously arrived at—giving us 64×3, or 192. These 192 squares, however, do not exhaust the type C. I am indebted to Mr. James Cram, the author of an ingenious little book on magic squares, for five other squares of this type, each of which may be transposed in all the ways above described excepting two, thus producing 16 varieties of each instead of 64. This gives us 5×16, or 80 additional squares—making up the 272. I give below the analysis of one of these 80, as it is very peculiar :

1	5	12	16		1	1	4	4		0	4	8	12
15	11	6	2		3	3	2	2		12	8	4	0
14	8	9	3		2	4	1	3		12	4	8	0
4	10	7	13		4	2	3	1		0	8	4	12
	(10)					(11)					(12)		

The skeleton squares (11) and (12), it will be observed, both sum wrongly in their diagonals. Nevertheless, on combination, the resulting square is found to be correct ; the errors having an opposite character, and neutralising one another.

The decomposition of squares is easily effected by aid of little tables like the following :—

	1	2	3	4
0	1	2	3	4
4	5	6	7	8
8	9	10	11	12
12	13	14	15	16

	1	2	5	6
0	1	2	5	6
2	3	4	7	8
8	9	10	13	14
10	11	12	15	16

	1	3	5	7
0	1	3	5	7
1	2	4	6	8
8	9	11	13	15
9	10	12	14	16

Find the number you wish to decompose in the table belonging to the series, and in a line with it will be found at the top the number for one of the skeleton squares, and at the left-hand side that for the other.

A PERPETUAL CALENDAR.

Mr. C. L. Prince, of Crowborough, has sent us a very simple perpetual calendar devised by him a few years ago, which avoids the necessity of committing to memory the rather complicated rules given in Mr. R. W. D. Christie's letter in our last number.

The accompanying block may be cut out and mounted on two pieces of cardboard. The inner circle of Domi-

nical letters and days of the month should be mounted on a circular piece of cardboard, affixed by a paper-fastener or button and thread through its centre to a larger piece of cardboard, on which the outer circle con-

taining the days of the week and months should be gummed concentrically with the inner disc.

Mr. Prince's rules for finding the day of the week corresponding to any date in this century, are as follows :—

A List of Sunday Letters from A.D. 1800 to 1899.

D	1	29	57	85	
C	2	30	58	86	
B	3	31	59	87	
A	4	32	60	88	G
F	5	33	61	89	
E	6	34	62	90	
D	7	35	63	91	
C	8	36	64	92	B
A	9	37	65	93	
G	10	38	66	94	
F	11	39	67	95	
E	12	40	68	96	D
C	13	41	69	97	
B	14	42	70	98	
A	15	43	71	99	
G	16	44	72		F
E	17	45	73		
D	18	46	74		
C	19	47	75		
B	20	48	76		A
G	21	49	77		
F	22	50	78		
E	23	51	79		
D	24	52	80		C
B	25	53	81		
A	26	54	82		
G	27	55	83		
F	28	56	84		E

"Rotate the Circular Index until you place the Sunday Letter for the Year under the Month you require, when it will show the day of the week for any day in that month, and will thus serve as a Calendar for any number of years. The Letters in the first column of the adjoining Table are the Sunday Letters for each year in the same line."

"In the case of Leap Year (to February 29th only), make use of the first of the two Sunday Letters for that year; thus, for the year 1888, A would be the letter to February 29th, and G for the remainder of the year."

"Each Letter in sixth column denotes that all years between it and that in column one are Leap Years. This table renders the Calendar useful for ascertaining the day of the week of any date during the present century."

The days of the week for next century may be at once determined by finding the day of the week for the corresponding day of this century, and going back two days; thus, the 1st of January 1801 was on a Thursday, and the 1st of January 1901 will be on a Tuesday. The days of the week for last century, from the 14th of September 1752, when the eleven days were dropped, till the end of the century, may be determined by finding the day of the week for the corresponding day of this century and going forward two days; thus, the 1st of January 1853 was on a Saturday, and the 1st of January 1753 was on a Monday.

To determine the days of the week for the twenty-first century we must go backwards three days in the week from the corresponding day of this century, for the year 2000 will be a leap year, while the years 1900, 2100, &c. are not leap years. For the twenty-second century the rule will be—go backwards five days in the week, which is the same thing as going forward two days.

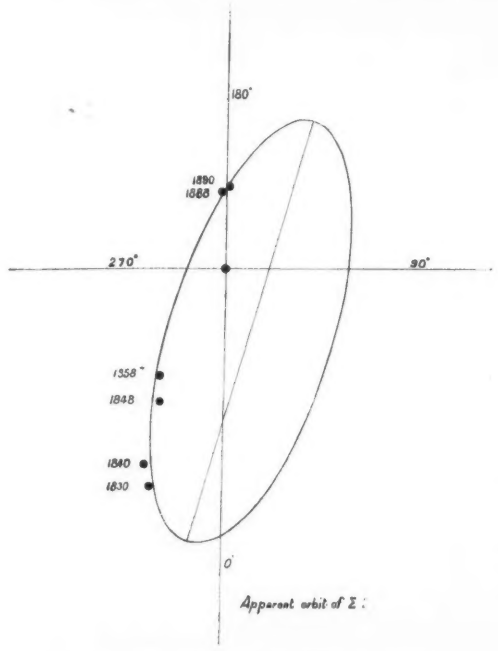
In the twenty-third century, people will keep the tetracentenaries, or fourth-century celebrations of events which have happened in this century, on the same day of the week as that on which they actually happened or are supposed to have happened.

In future all tetracentenaries will occur on the same day of the week as the event they commemorate, for a period of 400 years must always include one year which will divide by 400 without leaving a remainder.

NOTE ON THE ORBIT OF THE DOUBLE STAR Σ 2.

By S. W. Burnham.

It would have been hardly possible to get even an approximate orbit of this star with the measures made during the fifty years following its discovery. At the time of the first observations by Struve it was comparatively easy to measure; but the stars slowly approached each other, and after 1858 the measures are few and very uncertain. Many of the estimated, or partly measured position-angles are obviously very largely in error, judging from the earlier measures which may be assumed to be reasonably exact, since the distance between



the components was sufficient to make it easily measureable with a moderate aperture. No attempt, so far as I am aware, has been made to compute an orbit from these measures. The entire angular motion from 1830 to 1858 was only 12°. The principal change was in the distance, which at the last-named date was only one-half that at the time of the first measures by Struve. So far as these observations are concerned, they could be as well repre-

sented by rectilinear motion as any other. The probabilities, however, in the case of a pair of this kind are greatly in favour of the relative change being due to orbital motion, and it was safe to assume that these stars were far more likely to be physically connected than that one was drifting past the other from proper motion.

In using the 36-inch telescope for double star work, as far as time would allow, I have looked up and re-measured some of the old pairs which have either been single for many years, or so difficult that they were practically non-measurable with ordinary telescopes. The result has been that in a number of instances a single set of measures of each star made it possible to get a reasonably accurate idea of the periods. In this case, the change in angle since the last measure of Otto Struve amounts to over 150°, so that the apparent path of the companion in the future is confined within narrow limits. I had overlooked, when placing this star on the working list, an excellent set of measures by Tarrant in 1888, made with a 12-inch reflector. These measures are remarkably accordant with my own made with a much more powerful instrument.

I have taken all the measures of this pair which can be used in any investigation of its motion, and laid them off accurately to scale (5 inches=1″), with the distance to the second decimal place, and the angles to the nearest tenth of a degree. As nearly as possible through these positions an ellipse has been drawn which will make the areas proportional to the times, and allow of the minimum correction of the observed angles and distances. The figure shown on the accompanying diagram is in substantial compliance with these conditions, and the errors of observation are practically insensible in measures of this kind.

The following are the measures made use of:—

1830	·85	341°·5	0″·81	Σ	5 n
1840	·56	338°·4	0″·74	OΣ	3 n
1848	·22	334°·9	0″·52	OΣ	6 n
1858	·50	329°·3	0″·44	OΣ	10 n
1880	·58	Certainly single		β	
1888	·09	182°·8	0″·3±	T	3 n
1890	·82	177°·1	0″·29	β	3 n

During the interval of about thirty years, in which there are no measures, it was frequently noted as single, and doubtless was apparently so with the instruments used; but much of the time the distance must have been at least 0″·2, and of course this would have been noticed with a larger aperture. I have given above my own negative results in 1880, as the observation was made with the 18·5-inch of the Dearborn Observatory, and the distance must have been very small to have escaped detection. According to this orbit, at that time the distance should have been a little less than 0″·15, and so slight an elongation probably would not have been noticed with that aperture. With the Lick telescope it would have been measureable at all times.

It is not claimed that anything more than approximate results can be derived from these investigations, but the graphical method is probably as good as any other with the present data. It is evident that the period is a long one; and according to this ellipse it would be about 450 years. We have also the following:—

Maximum distance - - -	0″·98
Minimum distance - - -	0″·13 (1876)
Major axis - - - -	1″·55
Minor axis - - - -	0″·58
Position angle of major axis -	164°·5

The change for some time will be mainly in distance. In about five years more it should be 0″·4; and it will then be easily measurable with almost any instrument. Frequent measures of a pair of this class are not necessary. A few careful sets of measures every five or ten years will be all that is required for any purpose.

Letters.

[The Editor does not hold himself responsible for the opinions or statements of correspondents.]

HOW DID HE FIND THE WAY?

To the Editor of KNOWLEDGE.

DEAR SIR,—If it will not trespass too greatly on your space, I should be glad if you could afford room for the following incident:—

In August 1889 a gentleman and his family removed from Stockport to St. Leonards-on-Sea, taking with them a fine Tom cat they had had several years. Tom seemed restless at this change of quarters, and, after about a fortnight, disappeared. Some weeks later, Tom's master received a letter from one of his sons who was resident in Stockport to the effect that Tom had been seen by the neighbours prowling round his old home. Shortly after, he disappeared again, and about three weeks later again arrived at St. Leonards in a very deplorable condition—reduced to a mere skeleton. His master, on returning home late one night, found the cat on the door-step, and was welcomed by him with every possible demonstration of delight. Tom has been very feeble ever since, and is most unwilling to leave the house.

The puzzle is, how could the cat find his way from St. Leonards to Stockport, a distance of 200 miles, seeing that he was brought by train, and was shut in a sack for most of the way. And to make the puzzle yet more difficult, the railway journey was necessarily broken in London, and the cat was conveyed from the northern to the southern station in a cab. The return journey to St. Leonards one can understand, but how did he find his way to Stockport?—E. W. MAUNDER.

[The weak link in this remarkable story seems to be that Tom was only seen and recognised by the neighbours, prowling round his old home. A recognition by the son would have been more satisfactory, especially if he had marked Tom.—A. C. RANYARD.]

I have made inquiries, and find that—

1. The intimation that the cat had appeared in Stockport was an independent one, *i.e.* before the people at Stockport knew that the cat had been missed at St. Leonard's.

2. The cat was away just over seven weeks in all, and was seen at Stockport during the middle week of the seven.

3. The cat was seen, recognised, fed, and taken care of by the next-door neighbours of its owners. The son resident in Stockport was unfortunately unable to come to identify the cat until after it had set out on its travels again.

4. The cat, which was formerly very fine and healthy, has suffered ever since from severe bronchitis.

5. Neither the owners of the cat nor the neighbours have the slightest doubt as to its identity. Indeed they are disposed to be rather indignant if it is hinted that there may be the possibility of a mistake.

If it was *not* the same cat that turned up at Stockport, then there certainly were some remarkable coincidences.

E. W. MAUNDER.

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THE MILKY WAY IN THE SOUTHERN HEMISPHERE.

By A. C. Ranyard.

THE plates illustrating this paper have been made from photographs taken by Mr. Russell, Director of the Sydney Observatory, New South Wales. They will bear close examination with a magnifying glass, and the sharp images of the small stars show how very accurate and steady must have been the motion of Mr. Russell's driving clock, which kept the camera directed to the stars during the exposures by a motion about the polar axis of the instrument in a contrary direction to the earth's motion about its axis. The driving clock was controlled by an electrical apparatus, contrived by Mr. Russell, which connects it with a governing clock and two heavy pendulums. These photographs form a very satisfactory certificate of Mr. Russell's method of electrical control, and show that the differences of refraction due to the changes of the altitude of stars during long exposures may be more accurately compensated for than had hitherto been supposed.

The scale of these star-pictures will be best appreciated from photograph No. II., which shows the greater part of the lower or southern portion of the constellation of Orion. The three stars at the bottom of the picture form the belt of the constellation giant. The three stars in a nearly vertical line above them, with the Great Nebula about the central star, form the sword; and the two stars on either hand towards the top of the picture form the feet of the giant. Unfortunately the printers have turned this picture, as well as No. III., with the south point at the top instead of at the bottom of the pictures, as in Nos. I. and IV.

Picture No. II. enables us to show that the central line of symmetry of the Orion Nebula, towards which the great curving structures springing from the neighbourhood of the trapezium are synclinal, is, as nearly as one can judge, at right angles to the medial plane of the Milky Way. A reference to any good star map or globe showing the Milky Way, will show that the line passing through the stars of Orion's Belt is nearly parallel to the plane of the Milky Way, the axis of symmetry of the Great Nebula is nearly square to the line of the belt, and consequently at right angles to the general plane of the Milky Way. Another fact worth mentioning, as showing an apparent symmetry with regard to the plane of the Milky Way, is that the two remarkable nebulous lines joining stars in the Pleiades Nebula* are approximately parallel to the general plane of the Milky Way, though neither of the lines are quite straight or parallel the one to the other. The edges of the great curving structures of the Orion Nebula are all harder on the inside towards the axis of symmetry which passes through the trapezium, and softer or more nebulous on their outer or convex edges. The scale of picture No. II. is too small to show this, though it is partly shown on the larger picture published in the May number of Knowledge for 1889, and referred to in the accompanying article on the Nebula. It is still more evident on the original negatives, and would alone prove, even if we knew nothing of the symmetry of curvature of the tree-like structures which spring from the trapezium region, and of the canopy which overhangs the whole nebula, that there are mighty forces acting towards and away from the axis of symmetry—that is, parallel to the plane of the Milky Way.

* *See* Knowledge for January 1889, pp. 69–70.

It will be remembered that both the Pleiades Nebula and the Orion Nebula lie a little to the south of the Milky Way, at about the same distance from its medial plane. There seems to be no obvious connection between the plane in which the Great Nebula in Andromeda lies and the plane of the Milky Way, and the same remark applies with regard to the Ring Nebula in Lyra. The elliptic patches of light into which these nebulæ project have not their major and minor axes parallel and perpendicular to the medial line of the Milky Way, as would be the case if the planes of these nebulæ were either parallel or perpendicular to the plane of the Milky Way; but though these two nebulæ are both on the borders of the Milky Way, the spiral nebulæ do not seem to be associated with the Milky Way in the same intimate manner that the other large and irregular nebulæ are; thus the spiral nebulæ in Ursa Major and Canes Venatici are at some distance from the Milky Way, and their planes are evidently not parallel to one another. Even the small elliptic nebulæ H.V. 18, discovered by Miss Caroline Herschel close to the great Andromeda Nebula, evidently lies in a different plane from the Great Nebula.

Picture No. I. represents the part of the Milky Way in Sagittarius photographed by Mr. Barnard, which we reproduced in the July and August numbers of Knowledge. The differences between Mr. Russell's photograph taken at Sydney on the 2nd October 1890, and Mr. Barnard's photograph taken at the Lick Observatory on the 1st August 1889, are very curious and worthy of close attention. Both photographs were taken with six-inch portrait lenses of about 31 inches focus, so that they are on about the same scale, and the pencil of light falling on the sensitive plate was in each case of about the same intensity. Mr. Barnard's photograph was exposed for 3h. 7m., and Mr. Russell's for 4h. 2m. Nevertheless, Mr. Barnard's photograph shows much more of the nebulous structure of this region of the Milky Way than Mr. Russell's. This might be due to differences in the method of development, or to a difference in the sensitiveness of the plates used (Mr. Russell seems to have used Ilford extra rapid plates; and Mr. Barnard, I believe, used some plates prepared by the American Seed-plate Company). But it is remarkable that the relative brightness of the nebulous areas on different parts of the plates do not correspond with one another on the Russell and Barnard plates. The reader should refer to the large picture from Mr. Barnard's negative published in the July number of Knowledge in order to follow what I am about to say. He will then see that there are great differences in the brightness of different parts of the nebulous structure, while the relative brightness of the stars is much the same on both the plates—with one notable exception, which it may be well to draw attention to before referring to the differences in the brightness of the nebulous light.

In Mr. Barnard's picture there are two clusters of stars near to the edge of the field at the bottom of the plate. Only one of these clusters, viz. that to the right hand or western side of the field, is shown in Mr. Russell's plates. The almost equally brilliant cluster near to the bottom of Mr. Barnard's plate is missing in the Sydney photographs, for Mr. Russell has sent over silver prints from two negatives, one taken on the 17th September 1890, and the other, reproduced in our picture No. I., taken on the 2nd October 1890. I at first thought it possible that both photographs might by mistake have been copied from the same negative, and that the differences between the Lick and Sydney photographs might possibly be due to inequalities in the sensitiveness of different parts of the film of Mr. Russell's plates. But it is evident that no

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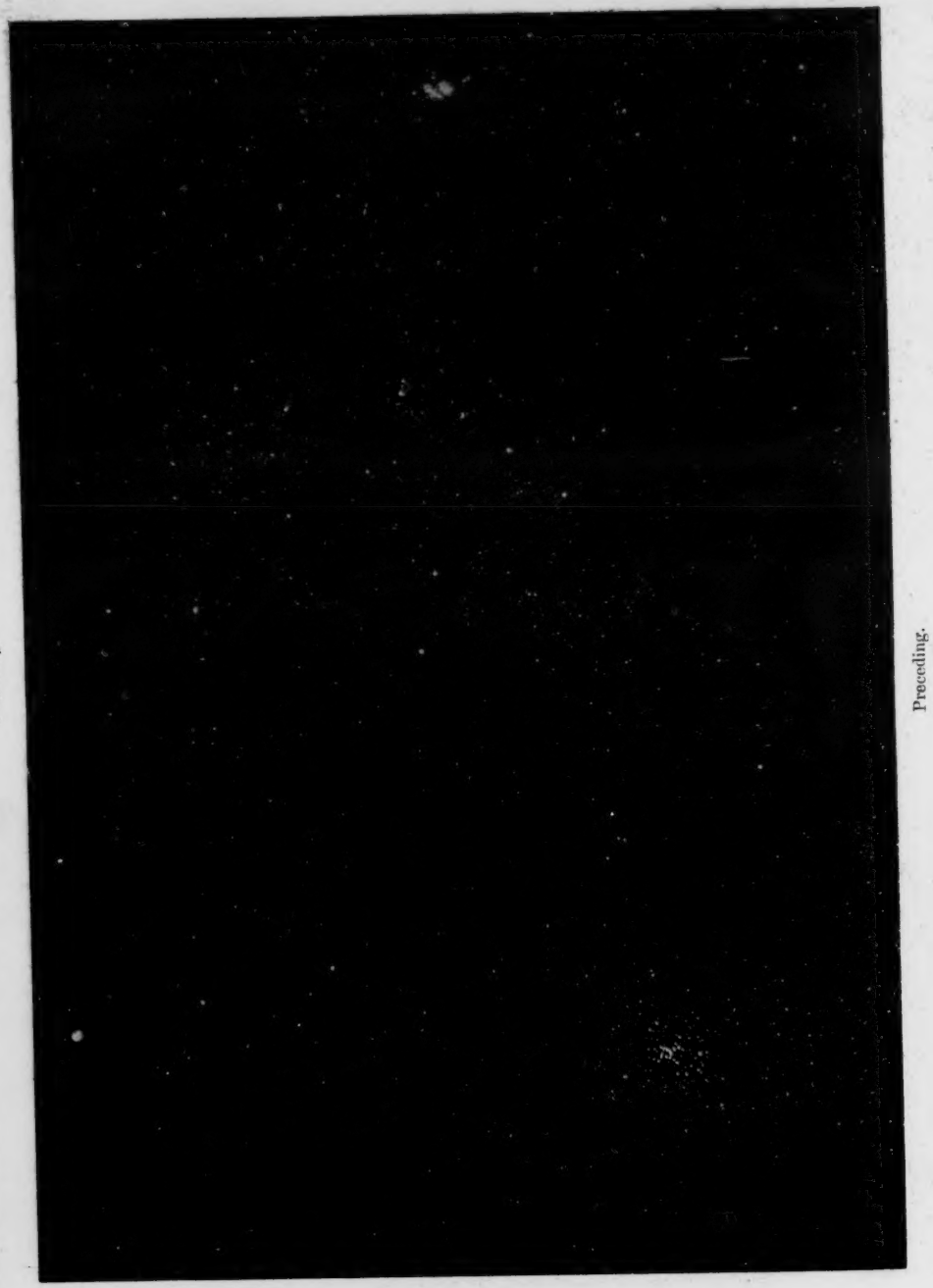
North.

Following.



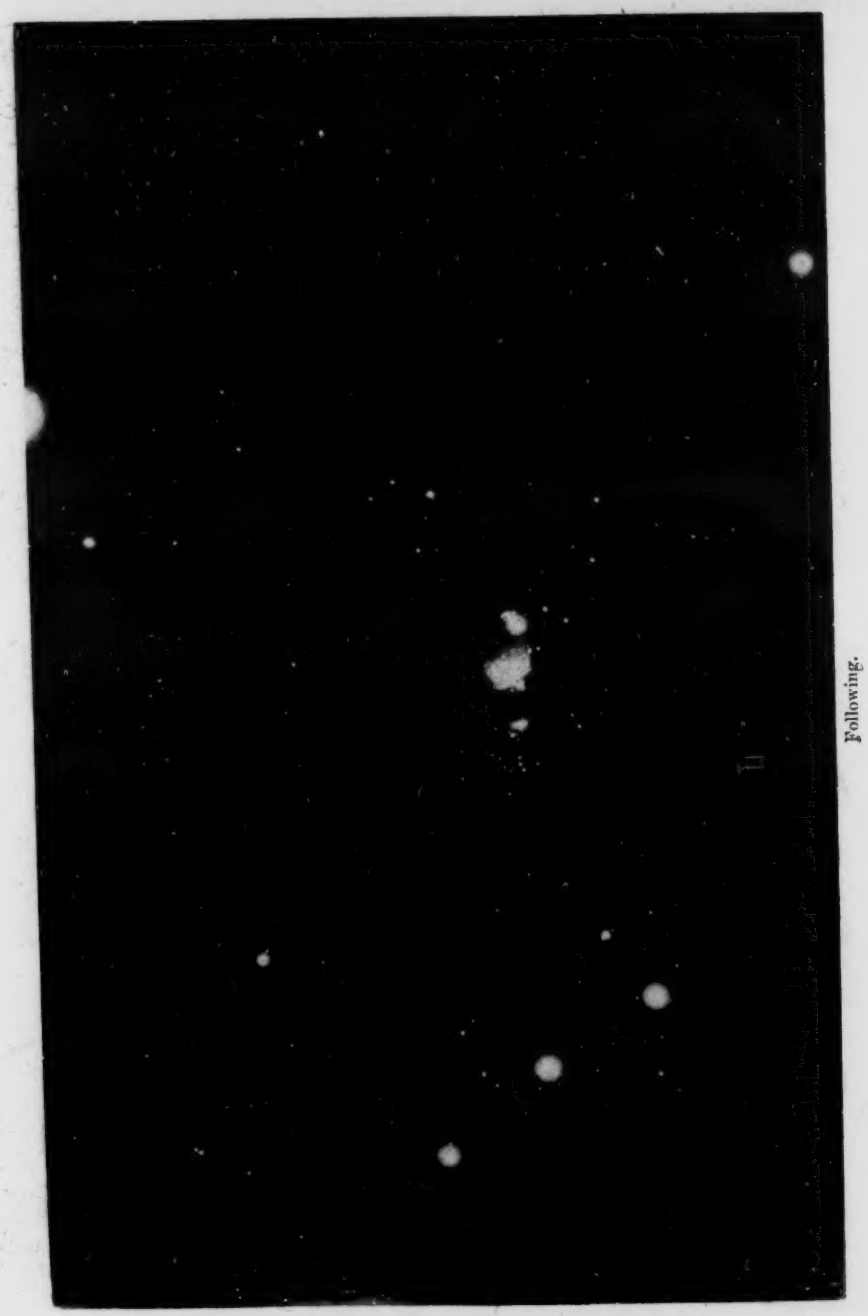
Preceding.

I.—Photograph of the Milky Way in Sagittarius.

Taken by H. C. Russell, Director of the Observatory, Sydney, N.S.W., 2nd Oct. 1890. Exposed 4h. 27m.

South.

Preceding.



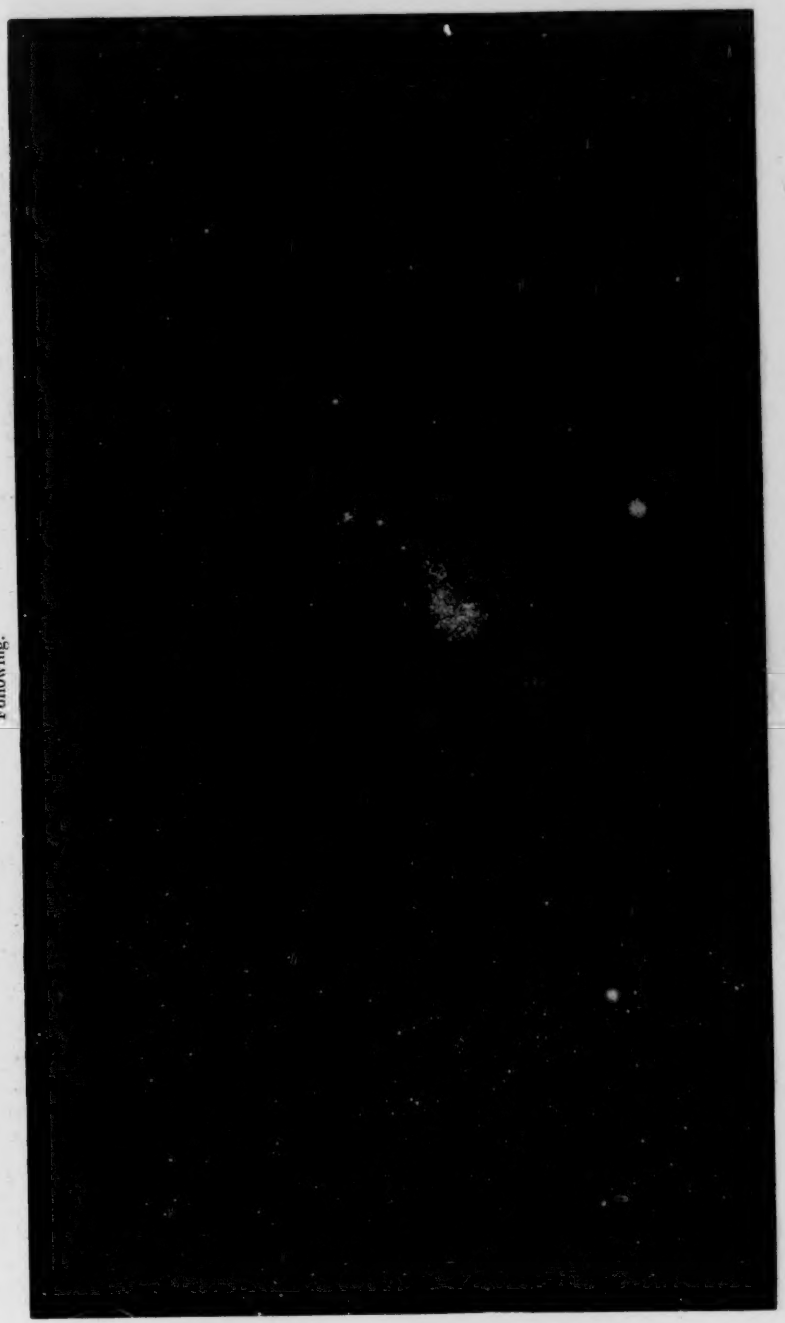
Following.

II.—Belt and Sword of Orion, showing Great Nebula.

From a negative taken by H. C. Russell, 19th Oct. 1890. Exposure, 4h. 5m.

Preceding.

III.—Photograph of the Nebecula Major.
Taken by H. C. Russell, 17th Oct. 1890. Exposure, 7h. 3m.



Following.
Following.

Preceding.

IV.—Photograph of the Nebecula Minor.
Taken by H. C. Russell, 14th–15th Oct. 1890. Exposed 8h.

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such mistake can have been made, for Mr. Russell's photograph of the 2nd of October contains a trace made by the planet Mars during the exposure, while the photograph we have here reproduced does not. There seems, therefore, to be no doubt that we have two independent photographs, and we seem to have evidence of very rapid change in the brightness of the southernmost of these two star-clusters.

While the nebulosity in the upper part of Mr. Russell's picture corresponds generally with that in the upper part of Mr. Barnard's picture, making allowance for differences of sensitiveness of the plates used, that in the middle and lower parts does not. The brightest* nebulous region in Mr. Barnard's picture is on the right-hand side of the tree-like form which stretches across the middle of the plate at the base of its lowest right-hand branch. But this bright region is entirely wanting in Mr. Russell's photographs, as also is the very bright nebulous region to the left-hand side of the base of the great tree-like structure. It is, of course, possible that such differences might be due to the nebulosity of the upper part of the tree-like structure being caused by a stippling produced by larger stars than those which give rise to the nebulous appearance on the lower part of the plate, and that while the small stars have left their trace on Mr. Barnard's plate, only a larger grade of stars have impressed themselves sufficiently to leave a developable trace on Mr. Russell's plate. It is also possible that the lower part of the nebulous mass may shine with a different kind of light from that with which the upper part shines, and that while Mr. Barnard's plates were sensitive to both kinds of light, Mr. Russell's were only sensitive to the one. But it is also possible that we may here have evidence of the existence of a vast variable nebula which undergoes changes in the relative brightness of its parts with surprising rapidity.

I should like to call attention to the fact that the branching tree-like form shown in Mr. Barnard's picture (the upper part of which appears in Mr. Russell's pictures) seems to afford us evidence of the projection of matter into a resisting medium just as certainly as the tree-like forms in the great Orion Nebula and the tree-like forms † in the Corona bear witness to explosions on a colossal scale, which have taken place below their bright bases, causing a stream of matter to be projected upwards, which stream has subsequently been divided and its branches deflected from their original course by a resisting medium. If there were no resisting medium and the only force acting on the projected matter was gravity towards the region from which the explosion took place, the streams would have the form of trajectories, and they could only be projected into conic sections.

The actual existence of the great tree-form on Mr. Barnard's picture seems to be confirmed by the arrangement of the stars in lines along its branches, which is best shown in the small photographs published in the August number of Knowledge for 1890, and in the *Monthly Notices* of the Royal Astronomical Society for March 1890. To see the stars in these small pictures they should be examined with a magnifying glass. The brighter streams of stars will be recognized in Mr. Russell's photographs after they have once been seen in Mr. Barnard's.

We seem, therefore, to have evidence that there is a resisting medium which occupies a vast region of the Milky Way; and perhaps the whole nebulous circle which surrounds the sky is not one vast nebula. The resisting medium need not be gas; dust moving in space, or larger particles, would equally offer resistance. The variability in brightness over so vast a region, if substantiated by future photographs, will need us to assume the existence of forces travelling far more swiftly than light or electricity, and giving rise to the synchronous dimming or glowing of the light-giving matter.

Picture No. III. represents the Nebecula Major (the larger Magellanic cloud), taken by Mr. Russell on the 17th of October 1890 with an exposure of 7h. 3m. In a private letter enclosing me the silver print of this picture Mr. Russell says: "This negative has brought out the grandest spiral structure in the heavens. Herschel estimated this object to cover 48°. It is now shown to be one great spiral structure supported, as it were, by two smaller ones in which *stars only* are visible. One is situated on the *North following*, and the other on the *South preceding* side of the great spiral." These spirals are just visible on our plates, but they are not so well shown as on the silver print or on the transparency which Mr. Russell has kindly sent me.

Picture No. IV. represents the Nebecula Minor, taken on October 14–15, 1890, with an exposure of 8 hours. It is also spiral in structure, though not so clearly so as the Great Nebecula. Within it and around it are some curious streams of small stars, all of about the same magnitude. One of such streams, shown in our plate on the upper left-hand side of the chief cloudy mass, is like a double W.

Mr. Russell has also sent over a most interesting contact print from a negative of the Coal-sack region. Instead of being a completely closed space, it is seen to be open on the south side, and very numerous small stars are seen to be scattered over three-fourths of its area. It is only at its northern part that there is the absolute absence of stars so frequently referred to by Mr. Proctor.

Notices of Books.

The Physical Properties of Gases. By Arthur L. Kimball, of Johns Hopkins University. (William Heinemann, London. 1890.) Professor Kimball's book will be welcomed as giving, in simple, untechnical language, and in a manner easily to be comprehended by the non-mathematical section of the community, the reasoning by which physicists have been led from the properties of gases as they were discovered by experiment to the present generally accepted kinetic theory of their constitution. After having given in brief outline a history of the discovery of some of the more important gases and their behaviour under pressure and expansion by heat, Prof. Kimball deals with the easily condensable vapours, and the gases which do not obey Boyle's law. He then treats of air-pumps and diffusion and occlusion. Avagadro's law that equal volumes of all gases, under the same conditions of temperature and pressure, contain the same number of molecules, is illustrated and explained in a manner that must make it clear to the most obtuse. Crookes's experiments with high vacua and radiant matter are also well explained and illustrated. It is, perhaps, a pity that Prof. Kimball does not go a little further and show, as he might have done in an elementary manner, how the average velocity of the molecules of a gas may be determined from its density when the pressure which it exerts at a known temperature is measured, and how the number of molecules may be

* The nearly circular white patch at the top of the picture is, as was explained in the July number, due to an over-exposed image of the planet Jupiter, with the Trifid Nebula below it. Jupiter has moved away and the Trifid Nebula remains on Mr. Russell's pictures.

† See Knowledge for May 1889, p. 146.

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estimated from experiments on the diffusion and viscosity of gases. One of the most striking illustrations in the book is given in the chapter describing Sprengel pumps and high vacua. He says, "These high exhaustions are called by courtesy vacua, as they are the nearest approaches that physicists have been able to make to an absolute vacuum by the most refined methods known to science; and yet we should hardly call that space a vacuum in every cubic inch of which there are 350 million million molecules of the gas; and, according to the latest conclusions of the molecular theory, that is about the number in a cubic inch of air when it is reduced to one-millionth of the atmospheric pressure. To form some conception of the vastness of this number, we may consider that if, through the side of a little glass bulb of one cubic inch capacity that had been exhausted to this extreme degree, a hole were to be made through which a million molecules could enter in every second, it would take ten years for the pressure inside the bulb to be doubled; that is, for as many more molecules to pass through as those already contained in the bulb." Unfortunately, the book does not contain an index.

The Criminal. By Havelock Ellis. (Walter Scott, 1890.) During the last fifteen years the study of criminal anthropology has been carried on with great activity, and the rich harvest of facts which has already been collected is likely to lead to valuable conclusions, which may probably in the future enable us to deal more wisely with the criminal residuum that will always exist in a civilised society, in spite of school boards and free libraries and the other panaceas of certain theorists and philanthropists which are constantly proclaimed, like the patent medicines of the advertising quack, as a cure for all ills, social and political. The sentimentalist, who generally sympathises so much more with the notorious criminal than with his poor neighbour or relation, will be surprised to read what Mr. Havelock Ellis says about the physical sensibility of the criminal classes. He instances the wide prevalence of tattooing among them, frequently of the most sensitive parts, which are rarely tattooed amongst barbarous races, as showing the deficient sensibility of criminals to pain. Lauvergne mentions a convict who smiled with pleasure when, moxas having been applied to him, he saw his skin burning and heard it crack. Though loud in their complaints of trivial ailments, they are often unconscious of severe illness. At Chatham, in 1888, a prisoner dropped down dead on returning from labour; both lungs were found to be affected, and death was probably due to syncope. He had made no complaints to anyone. Prisoners will inflict severe injuries on themselves in order to gain some trifling object. At Chatham in 1871-72, 841 voluntary wounds or contusions are recorded; 27 prisoners voluntarily fractured a limb; and 17 of them had to submit to amputation; 62 tried to mutilate themselves, and 101 produced wounds by means of corrosive substances.

BIRDS AND BERRIES.

By the Rev. Alex. S. Wilson, M.A., B.Sc.

NATURAL History furnishes many curious illustrations of the mutual relationships subsisting between the animal and vegetable kingdoms. Of these we have remarkable examples in the well-known adaptations of flowers to the visits of insects. It is of the highest importance to a plant to have its seeds properly crossed, and this involves the transference of pollen from one flower to another of the same species. Insects frequenting flowers get dusted with this substance; they carry it with them to other flowers, where some of it adheres to the stigma prepared for its reception. The honey is not provided merely to gratify the bees, but as an inducement to them to visit the flowers and effect their fertilisation. A flower, in fact, is little more than an apparatus for securing cross-fertilisation. The scent and colour serve to guide the insects, while the shape of the flower is generally such that the bee cannot reach the honey without effecting the object for which it has been attracted.

Insects are not, however, the only animals to which plants are thus related. A considerable number of flowers appear to be adapted to birds rather than to insects. Humming-birds in America, sun-birds in Africa and India, the Malayan lories, and the Australian honey-eaters, visit flowers and effect cross-fertilisation very much as butterflies and bees do in Europe. The bird-fertilised class includes species of Fuchsia, Passiflora, Salvia, Abutilon, Impatiens, Lobelia, Marcgravia, Erythrina, and Cassia. Ornithophilous, or bird-fertilised, flowers are generally of large size, tubular in form, and secrete abundant nectar. Their colours are extremely brilliant, scarlet being perhaps the most frequent. Flowers of this description are rarely produced by herbaceous plants; they occur, as a rule, only on shrubs and trees.

Birds are employed to carry seeds much more frequently than for the transport of pollen; these bird-fertilised flowers have, however, a special interest as throwing light on the relations between birds and coloured fruits.

Fruits and seeds constitute a large proportion of the food of many animals; but if any animal were systematically to consume the seeds of a particular plant, the latter would run no small risk of extermination. To the animal itself this would be a serious misfortune if thereby it were deprived of its usual food. In the interest of the plant, as well as of the animal supported by it, some limitation to the consumption of seeds is necessary. Hence many plants conceal their seeds; in other cases these are obscurely coloured or encased in hard shells in order that at least some of them may escape being devoured. Other plants have been able to avail themselves of the services of animals, and can thus reimburse themselves for the loss they occasion. In some parts of Africa visited by the late Dr. Livingstone the grasses of the pastures frequented by herds of antelopes had their seeds adapted for dispersion by these animals. This arrangement is a mutual benefit, for in disseminating the seeds of the grass the antelopes unconsciously provide for their own future. The same thing may be said of birds which feed on berries and other succulent fruits. These are useful to plants in scattering their seeds, and in return they receive the soft, sweet pulp of the fruit, with the prospective advantage of a future crop from their own sowing. In plants which employ birds for their dispersion the adaptation is seen in the succulence, sweet taste, and bright colour of the fruit; and in the hardness, bitter taste, and emetic or purgative properties of the seed. There are two perfectly distinct objects to be secured; the attraction of the birds, and the protection of the seeds. Hence the succulent portion is not as a rule the seed itself, but some part of the pericarp or wall of the fruit. Berries have the pericarp entirely succulent, the hard seeds being embedded in pulp. Drupes, or stone-fruits like the cherry, have only the outer layers of the pericarp soft; the inner wall of the fruit, called the endocarp, is indurated, and forms the stone enclosing

the seed or kernel. Where the seed is so protected, it may itself be comparatively soft. The pomegranate and gooseberry are exceptional in this respect, that the testa, or outer layer of the seed, is developed in a succulent manner, the central core of the seed being, however, hard. The strawberry has the top of the flower-stalk very much enlarged; the edible portion is, in fact, formed from the thalamus (or spreading portion of the stalk from which the flower springs), the fruits being the little yellow seed-like bodies studded over its surface. The raspberry and bramble, on the other hand, have a dry, conical thalamus, on which are arranged a number of drupes corresponding in structure to the plum and cherry. In the mulberry the succulent portion is furnished by the calyx, and in the apple and fig the hollow receptacle, or flower-stalk, supplies the food material. From the small size of the seeds of berries we may infer that they are adapted to be swallowed along with the pulp. In the larger drupes the size of the stones and their rough or jaggy exteriors, as seen in the peach-stone, seem to indicate that the intention here is to induce the bird to fly to a distance with the fruit, and after devouring the soft portion, to drop the hard endocarp containing the seed. Where a fruit is not intended to be eaten it invariably acquires a hard and dry character.

Fruits adapted to birds are for the most part sweetly tasted. They contain, in addition to sugar, organic acids and essential oils, which confer an agreeable or even delicious flavour to the fruit, and constitute an attraction to birds as powerful as the nectar of flowers is to insects. If these qualities appeared too soon there would be a danger of the seeds being removed before they were ripe. Accordingly the fruit remains sour until the seeds are matured.

Succulent fruits are brightly coloured, to be easily recognised from a distance. Conspicuousness may be increased, as in the clusters of the grape, rowan, and elder-berry, by the massing together of the single fruits in groups, just as happens in composite and other flowers where the florets are crowded on a contracted inflorescence. The colour of the fruit in general presents a strong contrast to the foliage. If the fruit remain on the tree after its leaves have fallen, its colour will challenge attention all the more as the season advances. The scarlet fruits of the wild rose thus remain on the bare branches and present a most conspicuous appearance. When the ground is covered with snow, coloured berries form prominent objects in the country landscape. Artists frequently avail themselves of this contrast, and introduce into snow scenes a sprig of holly with its scarlet berries. When Zeuxis painted the picture that deceived the birds, he may have taken advantage of this contrast; but perhaps it was left for Father Christmas to reveal to us how perfectly the colours of fruits serve the purpose intended by nature.

The list of plants bearing coloured fruits includes the following, which are British:—berberry, bittersweet, spindle-tree, strawberry, rose, hawthorn, currant, rowan, dogwood, honeysuckle, whortleberry, cranberry, bearberry, holly, daphne, arum, asparagus, lily of the valley, yew, alder, sloe, bramble, elder, bilberry, crowberry, juniper, misletoe, and snowberry; besides these, may be mentioned the orange, tomato, fig, date, olive, and mango. The colours of fruits are less varied than those of flowers. Possibly this may arise from the circumstance that while it is of importance, as regards fertilisation, that insects should be able to recognise and distinguish different species of flowers, there is no necessity for birds to distinguish different fruits, or to confine themselves to one kind of fruit, as insects restrict themselves for a time to one species of flower. Although never variegated like flowers, many fruits under cultivation exhibit a twofold colouration; thus we have red and green gooseberries, purple and green grapes, red and white strawberries and currants, green and purple plums, &c.

(To be continued.)

VARIABLE STARS OF THE ALGOL TYPE.

By MISS A. M. CLERKE, *Author of "A Popular History of Astronomy during the Nineteenth Century," and "The System of the Stars."*

TEN stars of the Algol type are now known—ten stars, that is to say, which vary in light not so much physically as geometrically, through the accident of our point of view. They are, to begin with, very rapid binaries; but other binaries equally rapid shine with sensible constancy. It is only when the orbits of the revolving stars lie so nearly edgewise to the earth as to involve mutual occultations, that the peculiarity of a sudden loss of light at brief intervals is added to the peculiarity of composition into abnormally close systems. This has been spectroscopically demonstrated as regards Algol; and the other members of the class copy its phases with such fidelity as to leave no doubt that they too are genuine "eclipse stars." To argue the point would be to *enfoncer une porte ouverte*.

But this is not all. There are residual phenomena not amenable to explanation, simply by the recurring transits of a semi-obscure mass. Eclipses beyond question in all cases occur, and produce their due effects; yet complicated with others owning a different origin. Slight as these often are, their investigation offers perhaps the most promising clue to the labyrinth of stellar *physical* variability. For their evident connection with certain calculable phenomena of eclipse defines clearly the conditions under which they occur, and strongly suggests their origin through some form of mutual influence by closely revolving bodies, demonstrably of low average density. Moreover, the residual variations of Algol stars are of a nature tending to bridge the gap separating them from other variables. That is to say, the irregularities of light-change in the two orders show a very curious inverted correspondence, as if the same causes which produce darkening in the one set of objects produce brightening in the other. This unlooked-for circumstance can scarcely fail to become the guide to some important truth.

For eliciting it, however, observations both more detailed and better assured than those yet obtained are urgently needed; and it seems unlikely that they will be available until in this, as already in so many other departments of astronomy, the retina is superseded by the sensitive plate. The eye is nowhere more subject to illusion than in following the course of rapid luminous fluctuations; and its disabilities are not removed by any kind of auxiliary apparatus. Its very powers of adaptation, indispensable to it as a living organ, serve to impair its usefulness as a photometer. The stars, then, must register their own changes, and the method of photographic trails appears eminently suitable for the purpose of inducing them to do so. Professor Pickering has shown that comparative measures of different stars made in this way are reliable to about one-tenth of a magnitude; and discriminations based upon the varying width and intensity of successive sections of the same trail might be expected to reach a still higher grade of precision. Some practical difficulties would certainly have to be met, but probably none that would prove insuperable. Thus, an arrangement might be contrived for automatically, at fixed

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intervals, moving the telescope in right ascension by the width of its own field, while the plate was simultaneously shifted 20″ or 30″ in declination. A series of parallel trails would result, exhibiting with absolute fidelity the gradations of loss or gain of light by which an Algol star traversed a critical stage of its minimum. Although exposures covering the whole of any one minimum could rarely be obtained, the comparison of trail-pictures of various sections of successive minima would be almost equally instructive. The realization of this plan would seem to be of considerable importance for the study of variable stars, and may safely be left to the ingenuity of celestial photographers.

The eclipse-theory of stellar light-change possesses little elasticity. Its explanatory powers are well defined, and incapable of extension. In the first place, the progress of the variations which it can account for must be along a smooth curve. There can be no stoppages or interruptions. Again, the amount of change must be invariable. High and low minima might, indeed, very well alternate in the same star, although they have not yet been found to do so; but capricious deviations from the assigned measure of obscuration are inadmissible. They seem, nevertheless, occasionally to occur. S Cancri and U Ophiuchi have each been once observed to lose far more than the usual proportion of their light; and M. Dunér recorded, at Upsala, November 25 and December 7, 1890, two abortive minima (as they might be called) of Y Cygni, when the star dropped to the extent of scarcely five, instead of eight tenths of a magnitude.*

Besides these anomalies in the measure, there are anomalies in the mode of change, which are equally perplexing and more persistent. The curves graphically representing it are unsymmetrical in at least seven of the Algol stars.† Their light, in other words, varies at a different rate before and after minimum. This is obviously incompatible with the progress of an eclipse by a body moving in an approximately circular orbit. And marked ellipticities are impossible (as Professor Pickering long ago pointed out in the case of Algol) where the conjoined stars are in such proximity as to leave no room for considerable oscillations about a mean distance already perilously small.

But indeed no amount of eccentricity in the paths traversed could satisfactorily account for the observed peculiarity. To begin with, the retardation does not advance continuously; in three or four of the stars a pause is indicated, followed by a resumption of progress. Moreover, the observed irregularities are of an invariable type; they take the form of a delay in recovery after minimum. It is *always* the ascending branch of the curve which is lengthened.‡ In order to explain this remarkable circumstance on gravitational principles, we should need the wholly unwarrantable assumption that *all* the stars in question passed periastron before falling under eclipse. Such a concurrence of coincidences is of course highly improbable.

Look, besides, at the minimum curve of U Ophiuchi de picted in Fig. 1, from the mean of 295 observations by Mr. Chandler.

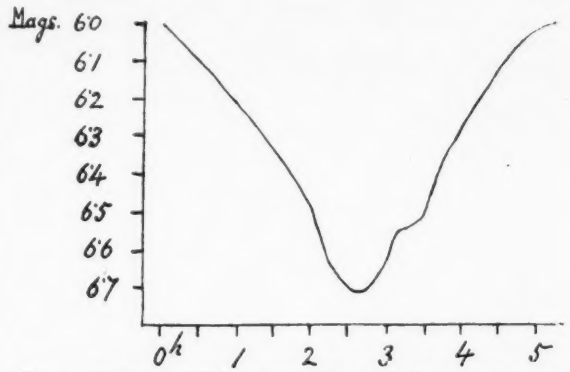


Fig. 1.—Minimum of U Ophiuchi. From 295 Observations by Mr. S. C. Chandler.

The singular inflection of its ascending line is vouched for, as an objective reality, by the independent determinations of Messrs. Chandler and Sawyer (*Astronomical Journal*, No. 177), and often appeared more conspicuous in individual phases than as it emerged from the average of many. The possibility of regarding it as an effect of orbital retardation is at once excluded by the fact that, on the whole, there is no delay. Accelerated progress before and after the pause is so exactly compensatory that the duration of recovery just equals the duration of decline, notwithstanding irregularities in the rate of one as compared with the rate of the other.

Here, then, evidently, we have a physical cause of obscuration co-active with the geometrical one, and travelling in its train. Conjectures as to its nature hence naturally associate themselves with the enormous tidal strains necessarily prevalent in systems of such peculiar construction as those of Algol and its congeners. From these extensive deformations of figure must result in both members of each revolving pair; but the effects upon light-change are not easily unravelled, and, indeed, depend to some extent upon what we know nothing about, the mode of axial rotation of the stars concerned. On this point the simplest, and perhaps most probable, hypothesis is that they have none relatively to each other. If this be so, they move as if *spitted* together; there is no travelling tidal wave, but each body has the permanent form of an ellipsoid with three unequal axes, the longest centrally directed towards the companion-star. The widest expanse of luminous surface would, under these circumstances, be presented to our vision a quarter of a revolution before, and a quarter of a revolution after each eclipse, when slight maxima should occur, with corresponding intermediate minima. And the matching of these theoretical by actual effects in λ Tauri, and perhaps also in Algol, is suggested by some recent observations of M. Plassmann, needing, however, to be confirmed before

* The exceptional minima of all these three stars have been recorded on excellent authority. U Ophiuchi is No. 6,162 of Schjellerup's Copenhagen Catalogue, and was suspected by him of variability, on the ground of his careful observation of it, June 9, 1863, as of 7·7 magnitude, while Lalande had put it at 6, Bessel at 7 magnitude. The regular course of change of the star, since ascertained by Mr. Sawyer, is from 6·0 to 6·7 magnitude once in every 20·3 hours. Of S Cancri the usual range is from 8·2 to 9·8 magnitude. Nevertheless, Schmidt observed it at Athens, April 14, 1882, to remain stationary for a whole hour at 11·7 magnitude (*Astr. Nach.*, No. 2,491). In his determinations of Y Cygni, Dunér used Chandler's comparison-star *p*, of 7·8 magnitude in the *Durchmusterung*, and exempt from any suspicion of change. At all the minima observed by Chandler and Yendell, Y Cygni sank decidedly lower than this star; but on the two occasions mentioned in the text Dunér found it to remain two steps (about one-fifth of a magnitude) brighter, and concluded, on apparently strong evidence, its phases to be inconstant (*Astr. Nach.* No. 3,011).

† These are: Algol, S Cancri, δ Libræ, λ Tauri, U Cephei, U Ophiuchi, and U Coronæ. S Antliæ will probably be added to the list; and we are unacquainted with particulars as to the phases of Y Cygni and R Canis Majoris.

‡ Although the curve of U Cephei (*see* Fig. 2) shows a slight pause *before* minimum, the whole time of recovery considerably exceeds that occupied by the decline in light.

much stress can be laid upon them. He, too, has recourse in a general way to the tidal rationale; and—what is more significant—ranks λ Tauri as a transition instance between Algol and β Lyræ, adding the remark that spectroscopic measures of its radial movements may help to elucidate the still unfathomed mystery of "short-period" variability. (*Astr. Nach.*, No. 3,016.)

A cause tending further to complicate tidal phenomena in sun-like bodies has been adverted to by Mr. Ranyard. It is this. A photosphere is probably a region of condensation, or the hottest region where matter can exist in a non-gaseous form. Consequently the temperature of the photospheric region is fixed. It may be regarded as an isothermal surface changing its level with local variations of heat. The photospheres, accordingly, of two adjacent radiating masses should bulge out somewhat, one towards the other. Deformations arising in this way in the Algol stars might be expected to become sensible to our perception—if at all—after a similar fashion to tidal deformations, namely, by maxima of lustre at elongations, minima at conjunctions.*

Neither source of disturbance, however, connects itself naturally with the enigmatical pause in recovery characteristic of this class of variables. And the late M. Klinkerfues's supposition of an atmospheric tidal wave following in the wake of the satellite, and bringing about partial obscurations through increased absorption, receives no countenance from the spectroscope. The light of U Cephei, it is true, turns ruddy as it fades; but not, it may safely be asserted, owing to this cause. The variations of its spectrum offer a tempting and hitherto unexplored field of study. Indeed, the star has of late in every way, especially in this country, been too much neglected. It is remarkable for a prolonged minimum, originally explained by Professor Pickering as due to a total eclipse by a large, semi-obscure satellite. But Mr. Chandler's observations showing variations of about two-tenths of a magnitude during the supposed totality, seem to compel recourse to some other hypothesis, if not to replace, at any rate to supplement the first. Two periods of the star being nearly equal to five days, only every second minimum can be followed at the same season of the year. Those of which the average course is represented in Fig. 2 occurred in the autumn. A shorter spring series of observations, also by Mr. Chandler, giving almost a dead level of least light, of close upon two hours' duration, claimed an inferior degree of authority (*Astronomical Journal*, No. 199). There is probably no real distinction in character between the alternating phases.

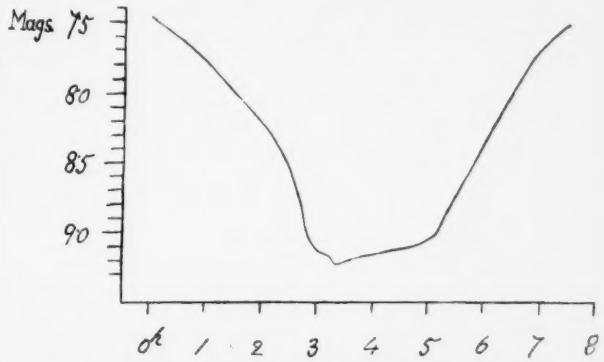


Fig. 2.—Minimum of U Cephei. From 159 autumn observations by Mr. S. C. Chandler.

The vicinity of this object to the pole renders it particularly suitable for "trailing" experiments, which might definitively settle the interesting question as to the true form of its light-curve.

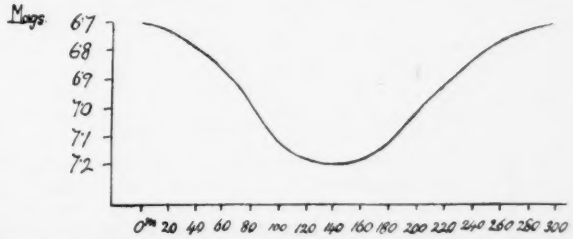


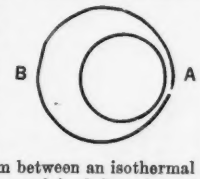
Fig. 3.—Provisional Minimum-Curve of S Antliæ (Yendell).

Since tidal effects grow, *cæteris paribus*, in the inverse proportion of the cube of distance, they ought, so far as they influence luminosity, to be most apparent in variables of the shortest periods, since these are proved by the proportionate length of their eclipses to be made up of the most closely contiguous pairs. But there is no sign that the subsidiary changes of Algol stars obey any such law. They are especially striking in S Cancri, with its relatively long period of nine and a half days.

* If we suppose the larger star to have given birth to the smaller eclipsing star, in a manner similar to that suggested by Prof. Geo. Darwin with regard to the birth of the moon from the earth, we should expect to find the larger star rotating on its axis faster than the smaller star completes a revolution about it; and the longest axis of the tidal ellipsoid would also, as in the case of the earth's tides, travel in advance of the line joining the centres of the two stars. Thus the larger star would present its minimum area before the time of inferior conjunction or central eclipse.

But there is another possible cause of variation in the light derived from the eclipsed star, which was not overlooked by Prof. E. C. Pickering in his remarkable paper on the "Dimensions of the Fixed Stars, with special reference to Binaries of the Algol type," published in the *Proceedings* of the American Academy, Vol. XVI., viz. the probable decrease of the brightness of the discs of stars towards their edges. Prof. Pickering says: "The presence of lines in stellar spectra leads to the belief that the stars, like our sun, are surrounded by an absorbing atmosphere. They also, therefore, probably resemble it in presenting a disc brighter in the centre than at the edges, owing to the greater thickness of the atmosphere and consequent greater absorption at the edges."

Prof. Pickering seems to have assumed that the decrease of brightness would be similar at both limbs, but with an egg-shaped star with the longer axis inclined to the line of sight the rate of increase of thickness of the absorbing layers would be different at the two limbs, and, under the conditions assumed above, we should have the decrease of light towards the preceding limb A more rapid than towards the following limb B; consequently the light of the larger star would recover its brightness more slowly just after central eclipse than it decreased before central eclipse.



According to my theory, the photosphere of a binary star would be intermediate in form between an isothermal surface and a surface of equilibrium, for, as explained in a former paper, we cannot suppose the photospheric clouds to be floating in an atmosphere. The particles must be falling under the action of gravity retarded but slightly, if at all, by gaseous friction, such as that which retards the fall of the particles composing a cloud in our atmosphere, but they would be retarded by the backward kicks of molecules evaporated towards the heated centre. Such falling particles would be finally evaporated at a level which would depend on the temperature of the region, as well as on their rate of falling; and since the acceleration of gravity would be similar at all places on a surface of equilibrium, and the temperature similar at all places on an isothermal surface, we should expect to find the falling particles glowing most vividly before their final dissolution in a stratum which would extend around the star as a thin shell intermediate in form between an isothermal surface and a surface of equilibrium.—A. C. Ranyard.

They are only doubtfully traceable in S Antliæ, notwithstanding the extreme swiftness of its revolutions. Mr. Yendell's provisional light-curve for the latter star, shown in Figure 3, regains its original level with scarcely perceptible retardation. Professor Paul, of Washington, the discoverer of this stellar prodigy, refrains for the present from pronouncing upon the reality of its suspected anomalies. Indications were, however, caught by him of a lagging after minimum, in the manner of U Ophiuchi and S Cancri, and he also records his impression of considerable inequalities both in the duration and intensity of its separate phases, a long, shallow curve occasionally replacing an undulation of wider amplitude and quicker accomplishment,* while both the loss and the gain of brightness appeared to proceed by accesses, rather than by an equable flow (*Astronomical Journal*, Nos. 215, 234).

The period of this star is by far the shortest known, either for a binary or a variable. According to Mr. Chandler, it retains its maximum brightness of 6·7 magnitude during 4h. 30m., sinks in about 1h. 40m. to 7·3 magnitude, and recovers with nearly equal rapidity; the whole period being of 7h. 46m. 48s. (*Astronomical Journal*, No. 218). Mr. Yendell's determinations, on the other hand, assign 5h. 10m. to the phases; but an eclipse extending over more than half the period of revolution is a manifest absurdity, only to be got rid of by doubling the latter,† and assuming the occurrence in each circuit of two mutual eclipses by equally bright stars. The reduction of light by about one-half at each obscuration renders this a plausible expedient, the adoption of which in nature can be tested by spectrographic means. A more powerful telescope than that at present in use at Potsdam would, however, be required to disclose the periodical doubling of spectral lines due to the possibly two-fold origin of the light we receive from S Antliæ.

A system is at any rate here presented to our consideration such as the boldest imagination could not beforehand have conceived. Even with a doubled period, the occulting twin-suns must revolve (if the data supplied by Mr. Yendell be accepted without modification) in so narrow an orbit relatively to their bulk, that the distance from surface to surface amounts to no more than $\frac{14}{100}$ of the distance from centre to centre. By reducing the time of light-change in accordance with Mr. Chandler's observations, a free space would be afforded of still much less than half the orbital span. The subsistence of such an arrangement cannot easily be reconciled with known mechanical laws; yet it seems undeniable.

NOTES FROM CAMBRIDGE.

By R. B. Johnson.

ANATOMY IN THE OLDEN DAYS.

THE turbulence natural to medical students and the popularity of Professors Macalister and Humphry combined to transform the new anatomical lecture-room into a scene of loud-voiced and inspiriting enthusiasm at Professor Macalister's opening lecture. He was commemorating the completion of the new buildings by giving a *résumé* of anatomical teaching in Cambridge from the days of the mediæval *studium generale* to the present time of elaborate sub-division.

The earliest record of a school of Physics at the University is in 1421, but the first definite provision for anatomical teaching was made by John Caius somewhat later in the same century. He was followed by W. Hardy in the sixteenth and by a brilliant galaxy of anatomists in the seventeenth centuries, of whom one instructed Newton, and another tried his hand at writing plays. From the time of Caius we were intimately connected with the Corporation of Surgeons in London, who sent us a scholar receiving £40 a year for his maintenance and £3 a year to provide himself with books. In order to qualify as a practitioner in those days it was necessary to have attended three dissections at which a body was opened, and "the physicians present discoursed at random concerning the interior."

The first separate professorship of Anatomy was founded at Cambridge in the year 1707, but the immediate effect of endowment appears to have been a cessation of all interest in the subject. It was the time of the Resurrectionists, however, and we read of the watchmen being allowed to search in Emmanuel for a missing body. This was illegal, be it remarked, and really an act of coercion, as may be seen from the following tale. A giant once died in Dublin, thereby exciting the desires of an anatomical professor and his students, to whom he said: "Gentlemen, I understand that your feelings are excited towards the seizure of this body, against which I must certainly counsel you. But in case your zeal should overcome your discretion, I will tell you the exact case of the law, which is, that you may take the body, but that for the removal of the least rag or shred of covering thereon you may be hanged. Therefore, if you *should* remove the body, be careful that it is utterly unclothed." Needless to say, that Professor was given the opportunity of experimenting upon that giant.

A more melancholy anecdote is associated with the memory of our own Professor Collignon, who once invited two friends to the dissection of a body, in which one of them recognised the features of an acquaintance. It was the body of Lawrence Sterne, "whose final return to his University formed a tragic ending to the sentimental journey of his life."

Professor Haviland made the first collection of anatomical specimens, while the first museum was founded by his successor Professor Clark, who raised it to be the first in the world. We have entered upon a goodly heritage, and, in the stimulating presence of Sir George Paget and Sir George Humphry, may we not learn to penetrate yet farther into those regions of knowledge where the unknown still far exceeds the known?

ARTIFICIAL COLD.

By Vaughan Cornish, B.Sc., F.C.S.

WITHIN the last twelve years the production of artificial cold has become an important industry. The principle of the methods employed has long been known, but it is only recently that the great practical difficulties of the problem have been overcome. The requisite impulse was given by the need of finding means for preserving meat in a fresh condition during its passage from foreign countries. For such purposes as this the freezing machines of Carré, which still figure in the ordinary text-books on Physics, are wholly inadequate. The problem was first practically solved by Coleman by the construction of the Bell-Coleman air machine, an apparatus so well thought out and perfected that in its first trial a cargo of meat of a value of £8,000 was transported across the Atlantic in a perfectly fresh condition. From 1879 the industry of

* The well-known correlation of short with sharp sun-spot maxima offers a curious analogy with these significant indications.

† As suggested by Mr. Backhouse in the *Observatory* for October 1890.

refrigeration has rapidly increased in importance as new applications have been perceived, and as further improvements in machinery have been effected. The subject has engaged the attention of many able engineers, and some three hundred patents have been taken out in connection with it. At the present time the production of low temperatures plays an important part, not only in the meat trade, but for the preservation of other perishable articles of food, as fish, eggs, and butter, in the brewing industry, and in the production of ice. New applications are being found every day, among which may be instanced the preparation of preserved fruits and similar processes, where crystallization from solution has to be effected. The important problem of the cooling of theatres is engaging attention at the present time, and will no doubt soon receive a satisfactory solution.

The principles involved in refrigeration present many interesting features which are scarcely touched upon in the ordinary works on heat. For the production of *high* temperatures it is usual to employ the force of chemical affinity; chemical combination in the process of combustion being attended with an evolution of heat. On the other hand, many chemical compounds are formed with *absorption* of heat; but these can only be produced in an indirect manner, and their formation cannot be employed for the production of low temperatures.

It is a curious circumstance that in order to understand the *rationale* of the refrigerating process it is necessary to consider, in the first place, the means of attaining high temperatures and the working of heat engines. By the burning of fuel in the furnace steam is produced in the boiler of a steam-engine, and by the changes of volume of the *working substance* (steam) a portion of the energy of heat is transformed into mechanical power. In other words, the energy which formerly consisted in that motion of minute particles which constitutes heat, is in the steam-engine converted into the form of energy which consists in the motion of a large mass of matter (*e.g.* of the piston or the fly-wheel of the engine). Briefly, in heat engines, of which steam-engines form one class, a calorific effect is converted into mechanical power. In refrigerating machinery, on the other hand, mechanical power is so employed as to yield a calorific effect; but in this case the calorific effect is *negative*, and the final result is the production of a *low* temperature. The refrigerating machine is not in itself a complete apparatus, since it requires to be *driven* by a steam-engine. In order, therefore, to attain logical precision in our view of the process of the artificial production of cold, it is necessary to consider as one complete system the combination of the steam-engine and the freezing machine. In this dual arrangement we start with the production of a high temperature in a furnace, and finally attain a very low temperature in the freezing chamber.

The working parts of the freezing machine are very similar to those of the steam-engine. In both there is a system of cylinders, pistons, and valves, and a working substance which undergoes alternately compression and expansion. In the Bell-Coleman machines the working substance is air. The process begins with the compression of air by the stroke of a piston in the *compression cylinder*. The power which drives this piston is obtained directly from the piston of the steam-engine. The compression cylinder is surrounded by a jacket in which cold water constantly circulates. The heat generated by the compression of the air is almost entirely taken up by the cold water. Thus we obtain air very little above the ordinary temperature, but under a high pressure. When the pressure is released the air expands. If the expansion be allowed to take place into a vacuum, then—as Joule first proved—no change of temperature takes place. But if the expansion takes place under such conditions that mechanical power is developed, the mechanical work is done at the expense of the heat of the expanding air, which consequently is chilled. This is what actually takes place in the *expansion cylinder*. The air, in expanding, drives a piston which is connected with the cylinder of the steam-engine in such a way that it aids the back stroke of the piston in the steam cylinder. Thus the frigorific effect is obtained in the refrigerating machine by an action which lightens the work of the driving engine. By means of this expansion the air is readily cooled to $-50°$ Fahr., or, if desired, to a still lower temperature. It was here that a great practical difficulty came in. Atmospheric air contains water-vapour, and at such low temperatures this was deposited in the form of hoar-frost. This frost or snow choked the valves and otherwise hindered the working of the machine. It was not found practicable to remove the moisture entirely before the admission of the air to the machine; and till the invention of Mr. Coleman the snow difficulty appeared to condemn the use of air as the working substance. This difficulty was overcome by the device of allowing a partial expansion of the air before it entered the expansion cylinder. This preliminary partial expansion is effected in sloping tubes placed in the refrigerating chamber itself. Under these conditions, the aqueous vapour deposits not as snow but in a mist or rain, and the moisture is run off by suitable taps placed at the bottom of the sloping tubes. The air thus freed from moisture enters the expansion cylinder to undergo the second and greater expansion by which the principal part of the frigorific effect is obtained.

The *cold air* freezing machines are those employed on board ship for the transport of meat from Australia, New Zealand, and America. The meat is placed in large chambers, the walls of which are double, the interspace being filled with wood charcoal as a non-conducting material. A jet of intensely cold air is delivered into the chamber at each stroke of the piston of the expansion cylinder, and the temperature of the chamber is thus kept at or near the freezing point during the whole voyage.

There is another important class of freezing machines, of which the ammonia machines are the most important type. In this second class the working substance is not a permanent gas such as air, but a substance (such as ammonia) capable of being condensed to a liquid by pressure, even at the ordinary temperature of the atmosphere. In these machines the frigorific effect is due in the first place to the heat absorbed by the vaporization of the liquefied substance; and secondly, as in the air machines, to expansion of the vapour. Volume for volume, the working substance exercises a much greater cooling effect in the ammonia machines than in the air machines. Consequently the machinery is more compact and more economical of fuel. An important point of difference between these two types is that whereas the air machines work with an *open cycle*, drawing in a fresh supply of material at each stroke of the piston, the ammonia machines work in *closed cycle*, the same working material going through the same round of changes over and over again. It will readily be perceived that this circumstance necessitates very different arrangements in the freezing chamber to those which have been described above, where the working substance itself is delivered from the machine and is the direct cooling agent. The refrigerating chamber connected with an ammonia machine is generally cooled by the circulation of a cold liquid in pipes, on a system similar to that employed in heating by means of

XUM

hot-water pipes. The liquid is some solution having a very low freezing-point, such as a solution of calcium chloride in water, *brine* being the term generally applied to such solutions.

In the ammonia machines a special cylinder for expansion is not required, the expansion being allowed to take place in long coils of tubing, which are placed in a bath in which the brine is kept circulating. From this bath the cold brine is driven by pumps through the system of tubes. An important advantage possessed by the ammonia machines is the fact that there is no moisture to be removed, and their construction is in consequence considerably simplified. Except on board ship they have undoubtedly an advantage over the air machines, and are coming daily into more general use. For marine *installations*—to use the trade term—the air machines are still preferred, owing principally to the fact that in case of accident the working substance could not be removed in the case of ammonia, the escape of which, owing to an accident in rough weather, would, moreover, be highly inconvenient.

An interesting application of cooling by means of brine has lately been made in mines. One of the greatest difficulties which can occur in the operation of sinking a shaft is that presented by a stratum of sand saturated with water. In more than one case this difficulty has been overcome by freezing the sand and water into a firm mass and then continuing the sinking operations as if the material were solid rock. The shaft having been sunk to the upper surface of the quicksand, a number of small bore-holes are made to the bottom of the stratum, and in these are placed tubes closed at the bottom, through which cold brine is circulated from a tank at the surface, which is cooled by an ammonia machine. In the course of a few days the quicksand is frozen to a solid mass, and the boring can be proceeded with. It will thus be seen that the production of artificial cold is an industry which, though still in its infancy, has already attained considerable importance. It appears likely that the next ten years may see a development scarcely less rapid than that of the last decade.

Whist Column.

By W. Montagu Gattie, B.A.Oxon.

THE following is an elementary explanation of the play of the hand published in the February number of Knowledge. For convenience of reference, the distribution of the cards is here repeated.

	Z D.—Kg., 5, 3, 2. C.—9, 8, 2, H.—10, 7, 4, 2. S.—10, 8.	
D.—8, 7, 4. C.—Ace, 6, 5, 3. H.—Kn., 9, 6, 5. S.—6, 2.	B Z turns up the King of Diamonds. A	D.—Ace, Qn., 9. C.—Qn., Kn., 7, 4. H.—Kg., 8, 3. S.—Kg., Qn., 5.
	Y D.—Kn., 10, 6, C.—Kg., 10. H.—Ace, Qn. S.—Ace, Kn., 9, 7, 4, 3.	

Trick 1.—A opens his longest suit, and, having four only, leads the lowest; Y, holding king and another, passes the trick; B plays his highest card, and Z his lowest.

Trick 2.—B, by opening with a small heart, shows his partner four at least of that suit. He might return a club; but this would not be quite prudent, and would also be likely to mislead A, inasmuch as the immediate return of a partner's suit is usually interpreted as a request for a third round on which to make a small trump. Z plays his lowest heart, and A his highest; and Y of course wins the trick with his ace.

Trick 3.—Guided by considerations which have been explained already, Y leads a trump, and, having three only, opens with the highest, so as to assist his partner (who has turned up the king) as much as possible. Z gathers that Y has not more than three trumps; for he would not lead knave from a long suit unless he held the four honours, or king, queen, knave, and two others, and both these cases are precluded by Z's holding the king. Z therefore *finesses* the knave, and at the same time takes occasion to commence an "echo" to the trump lead by playing the three instead of the deuce. When the deuce falls at trick 5, four trumps at least are marked in Z's hand.

Trick 4.—A continues with the queen of clubs in order to clear the suit. He knows that (unless the ten of clubs was the beginning of a call for trumps) Y has either the king single or no more; but in the latter case Y would pass a small card led (for, B having played ace on the first round, the king must then be with Z), and Z might win with a small card, retaining the command of the suit. B's three of clubs shows that A led from four cards originally; for, the deuce having fallen already, the four must have been A's lowest club, and, with five of the suit, he would have led the lowest but one. Z plays his lowest card, and, as this is the eight, it follows that the only other club he can hold is the nine (the knave, of course, being marked in A's hand).

Trick 5.—Y continues with his next best trump, Z completes his "echo," and the ten draws the ace from A.

Trick 6.—A is reluctant to enable Y to make a small trump on the clubs, and therefore returns the hearts. Holding two only, he returns the higher, and, as B and Z play six and four respectively, Y concludes that the three is in A's hand. He notices also that B, who led the five at trick 2, now plays the six, and therefore can only have had four hearts originally; so that, as A can have but one (the three), Z must hold the other two.

Trick 7.—After this third round of trumps, Y counts the hands, from inferences already drawn, thus:—Z's "echo" at tricks 3 and 5 has shown that he has the long trump; he has two hearts, from trick 6; and not more than one club, from trick 4. Therefore he must have at least two spades. A has the knave of clubs and one other (tricks 1 and 4), and the three of hearts; therefore he must have three spades. There remain for B two hearts, and either two clubs and two spades, or three clubs and one spade, according as Z has the nine of clubs or not.

Trick 8.—Z's proper lead is obvious enough. It would be fatal to lead up to Y's tenace in hearts, and he cannot do better than give his partner the best spade he has. A, holding king, queen, naturally puts on the queen to draw the ace and make his king good; but, looking at the great strength in spades declared in Y's hand, it would probably be better play to pass the ten, and that course would certainly save the game, whether Y *finessed* or not. A's play, however, is quite orthodox. Y's *coup* has already been fully explained. Its merit consists in his seeing that, except in the improbable case of Z's holding the best and

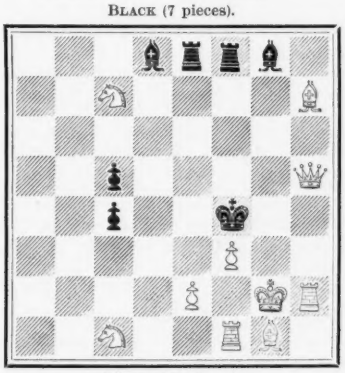
second-best hearts, the game cannot be won, and may be lost, if Z has the lead after becoming exhausted in spades. It should also be noticed that, while B and Z know perfectly well how the hearts are divided, A and Y are in the dark on this point. It is this uncertainty which deters A from leading a heart at trick 10, although he would thereby not only save but win the game, if B should be found with the best and second-best hearts. The maxim "Simplify the game as much as possible for your partner," has a scarcely less important correlative, "Place your opponent, whenever possible, in a difficulty." Y achieves both purposes in this instance by foregoing a single trick.

Chess Column.

By C. D. Locock, B.A.Oxon.

Problem by T. Taverner.

Black (7 pieces).



White (10 pieces).

White to play, and mate in two moves.

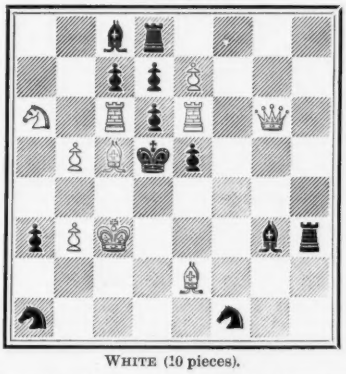
The above beautiful problem is taken from the *Liverpool Weekly Mercury.*

For the benefit of any who may find it too difficult, the following puzzle is given as an easier task.

Intending solvers need not be dismayed by its length: it is only necessary to go straight ahead. There are no variations, all Black's moves being forced.

Dedicated to Wordsworth Donisthorpe, Champion Stale-mater of the World.

Black (12 pieces).



White (10 pieces).

White to play, and *stale-mate his own King* in fifteen moves.

CHESS FALLACIES.

I. (Legal) That it is unlawful for a player to Castle on the Queen's side when his QR square and QKt square are commanded by opposing pieces. This is a delusion under which many fairly experienced players are known to labour. The law merely says that the *King* in castling must not do so from, over, or into check.

II. That a Rook and Knight are stronger than two Bishops. Many games have been lost through this fallacy. If the position is an open one with Queens on the board, the two Bishops if favourably posted will often win.

III. That the Queen is worth two Rooks and a Pawn. On the contrary, it can be proved that, when there are no other pieces on the board, two Rooks are nearly worth a Queen and Pawn. For, provided the K be secure from perpetual check, the two Rooks can double to attack the weakest Pawn, which can only be defended by the King and Queen. The two Rooks can then be exchanged for the Queen and Pawn.

IV. That P to R3 is a harmless "waiting" move. As a matter of fact, there is no such thing as a harmless waiting move in the earlier part of the game. The move P to KR3 is doubly disadvantageous. In the first place, the Pawn becomes a mark for the opponent's Queen's Bishop. And secondly, it makes it inadvisable afterwards to move the KBP, on account of the "hole" created at KKt3

V. The disadvantage of P to QR3 is a somewhat subtle one. Suppose, for example, the moves 1. P to K4, P to K4; 2. Kt to KB3, Kt to QB3; 3. B to B4, B to B4; 4. P to B3, Kt to B3; 5. P to Q3. Black now makes the "waiting" move P to QR3? White replies 6. B to K3. In positions of this sort, if the QRP were unmoved, Black would now retire the Bishop to Kt3. For if White exchanges, Black gets an open Rook's file. But in the present case, if he play B to Kt3, White exchanges with advantage. Or if he retire to R2, White gains two moves by taking it, leaving either the Kt or the R out of play. Black, therefore, is compelled either to exchange, and present White with an open KB file for his KR, or to play P to Q3, leaving White to exchange when it suits him.

CHESS INTELLIGENCE.

The Steinitz-Gunsberg match ended in a victory for Steinitz by 6 games to 4, with no less than 9 draws. The large proportion of drawn games seems to indicate a harder fight than the Steinitz-Tschigorin match of 1889, which the former won by 10 games to 6, with only 2 draws. On the whole, however, the result of the match is a confirmation of previous form, as shown in the recent drawn match between Tschigorin and Gunsberg. It is possible that Mr. Steinitz might have won both matches, had he chosen, by a somewhat larger majority. His invariable habit of adopting unsound defences undoubtedly caused the loss of several games in both matches. At the same time, it should be remembered that the intrinsic badness of his novelties is to a certain extent balanced by his greater familiarity with them. For these novelties are not played on the spur of the moment. On the contrary, they have generally been in Mr. Steinitz's note-book for many months before the match begins.

The Steinitz-Tschigorin Correspondence games have now been resumed.

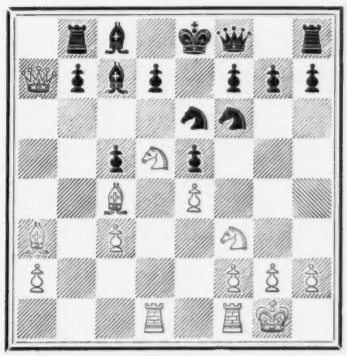
The following moves have been made since the publication of the Diagram in the January number:—

"Evans Gambit."

White (Tschigorin).	Black (Steinitz).
18. B to R3	P to QB4
19. QR to Qsq	Kt to B3
20. B to B4	B to B2
21. Kt to Q5	

Present Position.

Black.



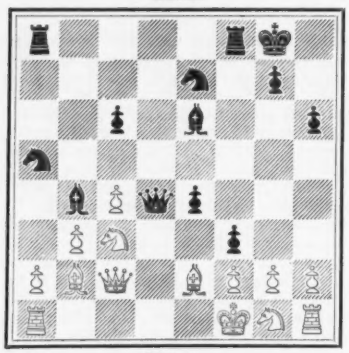
White.

"Two Knights' Defence."

White (Steinitz).	Black (Tschigorin).
19. B to Kt2	P to B 5
20 Q to B2	Q×P
21. K to Bsq	P to B6

Present Position.

Black.



White.

Mr. Gunsberg is expected to return before the end of February, but not in time to edit the Chess Column for March.

There are rumours of a match between Mr. Steinitz and Dr. Tarrasch of Nuremberg, who won the last two International Tournaments without losing a single game in either. The German amateur has signified his willingness to play; but it takes two to make a match, and Mr. Steinitz's consent is not always easy to obtain. Considerable self-denial in the matter of terms is essential on the part of anyone who wishes to be his opponent.

The match would be interesting as a contest between master and pupil; for Dr. Tarrasch is an exponent of that "modern school" of which Mr. Steinitz claims to be the founder. On that very account it is probable that, *if* the younger player should lose the match, he would be defeated by a more decisive majority of games than players of the opposite school, such as Tschigorin and Gunsberg, who attack Mr. Steinitz sometimes in a style of which he strongly disapproves, instead of waiting to be slowly attacked themselves, in accordance with Mr. Steinitz's principles.

The following game, played in the Manchester International Tournament of last year, is given, for want of a better, as a specimen of Dr. Tarrasch's style.

"Ruy Lopez."

White (C. D. Locock).	Black (Dr. Tarrasch).
1. P to K4	1. P to K4
2. Kt to KB3	2. Kt to QB3
3. B to Kt5	3. P to QR3
4. B to R4	4. P to QKt4 (*a*)
5. B to Kt3	5. B to Kt2
6. P to Q4	6. P×P (*b*)
7. Castles (*c*)	7. P to KKt3 (*d*)
8. Kt×P	8. Kt to R4 (*e*)
9. P to QB3	9. Kt×B
10. P×Kt	10. B to Kt2
11. R to Ksq (*f*)	11. Kt to K2
12. P to QKt4! (*g*)	12. P to Q3
13. Kt to Kt3 (*h*)	13. Kt to B3
14. QKt to Q2	14. Castles
15. Kt to B3	15. Q to Bsq (*i*)
16. Kt to R4 (*j*)	16. Q to K3
17. P to KB4	17. KR to Ksq
18. B to Q2 (*k*)	18. Q to B5!
19. B to K3 (*l*)	19. Q×KP
20. Kt to Q2	20. Q to K2
21. Q to Kt4	21. P to KB4
22. Q to R3 (*m*)	22. B to B3
23. Kt (Q2) to B3	23. Q to Kt2
24. QR to Qsq	24. P to QR4 (*n*)
25. P×P	25. Kt×P
26. Q to Kt3	26. Kt to B5
27. B to Bsq	27. B to K5!
28. Kt to Q4	28. P to Kt5!
29. Kt (R4) to B3	29. P×P
30. P×P	30. P to B4
31. Kt to K2 (*o*)	31. B to B7
32. R to Q5	32. Q to B2
33. Resigns	

Notes.

(*a*) An obsolete and not very satisfactory defence, revived probably with the view of getting "out of the books."

(*b*) Best. Mr. Steinitz recommends here 6. . . . Kt×P, overlooking the winning reply 7. B×P ch! K×B; 8. Kt×P ch, K to K3; 9. Q×Kt, P to B4; 10. Q to B3, and if 10. . . . P to Kt5, 11. Q to KKt3, with a strong attack whether Black take the KP or not. If, for instance, 11. . . . B to Q3, White wins by Q to Kt4 ch!

(*c*) Better than retaking the Pawn immediately.

(*d*) The result of twenty minutes' consideration, and very likely therefore the best move. If 7. . . . Kt to B3, 8. PK5; while 7. . . . B to B4 (or B to K2) would be met by 8. P to QB3 with the advantage.

(*e*) Dr. Tarrasch afterwards thought that 8. . . . B to Kt2 at once would be better. White's reply would have been P to QB3, providing a loop-hole for the escape of his Bishop.

(*f*) To prevent PQ4.

(*g*) The best move, creating two "holes" at Black's QR4 and QB4, which he will be able to command still further by a Knight at QKt3.

(*h*) White now proceeds to do in four moves what he could do perhaps equally well in two. He might play the other Knight to Q2 and Kt3. For after 13. QKt to Q2, B×Kt; 14. P×B, Kt to B3; 15. Kt to Kt3, Kt×P; 16. Kt to R5, followed by P to Q5, threatening to win a piece by Q to Q4, the position is probably worth the Pawn, and with Bishops of opposite colours there should be no danger of losing.

(*i*) Black's development is still difficult. If 15. . . . Q to Q2, White might reply 16. Kt to B5.

(*j*) To prevent P to KB4. Dr. Tarrasch thought that White could have maintained his advantage by 16. KKt to Q4, Kt×Kt; 17. P×Kt, considering the open QB file more than an equivalent for the isolated Pawns. Black, however, has two Bishops, and might continue with P to KB4.

(*k*) A bad move, of which Black takes immediate advantage. Kt to KB3 would be better. For if then 19. . . . Q to B5, 20. Kt (Kt3) to Q2, and the Q cannot play to Q6, on account of the reply K to B2.

(*l*) Having no way of saving the Pawn (Q to B2 would lose the QKtP) White compels his opponent to take it with the Queen.

(*m*) Q to Kt3 would be better, so as to be able to play P to KR4.

(*n*) Having secured himself from all attack, Black now proceeds to finish off the game in the most expeditious manner.

(*o*) The only move not to lose the Knight.

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